A JOURNAL OF NATURAL PHILOSOPHY, *CHEMISTRY,* AND THE ARTS.

VOL. VI.

Illustrated with Engravings.

BY WILLIAM NICHOLSON.

LONDON:

PRINTED BY W. STRATFORD, CROWN-COURT, TEMPLE-BAR, FOR

THE AUTHOR, No. 10, SOHO-SQUARE:

AND SOLD BY

G. AND J. ROBINSONS, PATERNOSTER-ROW.

1803.

ADVERTISEMENT.

THE Authors of Original Papers in the present Volume, are Mr. Frederick Accum; Mr. Irwine; Sir. A. N. Edelcrantz; Dr. T. C. Hope, F. R. S. Edinburgh; Anthony Carlisle, Esq.; M. B. Donkin; Mr. Ezekiel Walker; Thomas Thompson, M. D.; Dr. Boſtock; Mr. Dalton; Edward Howard, Esq. F. R. S.; Mr. A. Woolf; Andrew Duncan, M. D. F. R. S. Edinburgh; Dr. Prince; Mr. J. C. Hornblower, and Mr. G. Smart. Of Foreign Works, M. Regnier; Wiegleb; Guyton; Curaudau; Baunach; Payſſé; Parmentier; Steinacher; Schaub; Nicolai; Bouillon la Grange; Klaproth; Lomet; Tromſdorff; Seguin; Ritter; Thenard; Vauquelin; La Place; Guyton-Morveau; Von Hombolt; Raymond; Berthollet, and Haſſenfratz. And of English Memoirs abridged or extracted, Wm. Herschell, L. L. D. F. R. S.; Richard Chenevix, Esq. F. R. S. and M. R I. A.; Humphry Davy, Esq.; Wm. Hyde Wollaston, M. D. F. R. S.; Sir H. C. Englefield, Bart. F. R. S.; James Smithson, Esq. F. R. S. P. R. I.; Mr. James Woart; William Fairman, Esq.; Charles Hatchett, Esq. F. R. S.; The Right Hon. C. Greville, F. R. S.; Mr. John Dalton; Everard Home, Esq. F. R. S.; and Andrew Duncan, Junr. M. D. F. R. S. Edinburgh.

Of the Engravings the Subjects are, 1. Dr. Young's Apparatus for illustrating the Doctrine of Preponderance. 2. Apparatus for Experiments with Spouting-Fluids. 3. A Lock of Combination by M. Regnier. 4. Solar Phenomenon obſerved, by Sir H. Englefield, Bart. 5. Figure to ſhew the Proportion of the magnified Images of the ſame Star at different Times, by Dr. Herschell. 6. Sir A. N. Edelcrantz's Method of Raiſing Water in Worm-Tubs, Condensers, &c. 7. Eudiometric Apparatus, by Dr. Hope. 8. Veſſel for Inclosing

closing Anatomical Preparations, and other Objects. 9. Crystal of Electrical Calamine. 10. Curaudau's Furnace for evaporating by a regulated Economical Heat. 11. Mr. Woart's Method of fupporting decayed Timbers in Buildings. 12. Mr. Fairman's Method of extreme Branch-Grafting. 13. Cit. Lomet's Addition to the Sextant for meafuring Vertical and Oblique Angles at the fame Inftant, from an Air Baloon, for Military Service. 14. An Apparatus for drying Precipitates, and for Processes of Congelation, by Mr. Accum. 15. Hydraulic Combination for rendering the Atmospheric Pressure effective in Raising Water in Worm-Tubs, by Edward Howard, Esq. F. R. S. 16. Improvement in the Syphon, by the same. 17. A Rotatory Apparatus, by which the Power of a Steam-Engine is equalized without a Fly, and the Work may be stopped, or fet off in any Part of the Stroke, by Mr. A. Woolf. 18. Improvement in Mr. Ezekiel Walker's Reflecting Quadrant, by the Inventor. 19. A new Steam Valve, which indicates the Strength, and, without Attendance, regulates the Emission of Steam from a Boiler, by Mr. A. Woolf. 20. Mr. George Smart's Machine for Sweeping Chimnies without Climbing Boys. 21. An improved Chemical Furnace. 22. Two Views of the Great Fiery Meteor which appeared Nov. 13. 1803.

Soho Square, December, 1803.

TABLE OF CONTENTS
TO THIS SIXTH VOLUME.

SEPTEMBER, 1803.

OCTOBER,

OCTOBER, 1803.

NOVEMBER,

NOVEMBER, 1803.

DECEMBER,

DECEMBER, 1803.

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

SEPTEMBER, 1803.

ARTICLE I.

Experiments and Obſervations on the Compound of Sulphur and Phoſphorus, and the dangerous Exploſions it makes when expoſed to Heat. By FREDERICK ACCUM, *Practical Chemiſt and Teacher of Chemiſtry. Communicated by the Author.*

MARGRAFF, I believe, was the firſt who noticed the combination of phoſphorus and ſulphur; and Pelletier examined afterwards this compound, and pointed out ſome of its properties *. The latter philoſopher ſhowed, at leaſt, that the compound reſulting from the union of phoſphorus and ſulphur, in different proportions, is infinitely more fuſible than either of them taken ſeparately. Repeating the experiments of the French philoſopher, I had no apprehenſion that the combination of theſe two ſimple bodies was attended, under certain circumſtances, with conſequences which might prove fatal to the chemical operator. And it is with the view of preventing my brother chemiſts from falling a ſacrifice to unexpected dangers, that I ſhall relate an accident, which might have been attended with the moſt dangerous conſequences, before I ſtate the properties which characteriſe the compound which is the ſubject of theſe lines. The ac-

Compound of phoſphorus and ſulphur; when firſt examined.

not ſuppoſed to be attended with danger.

* Journal de Phyſique, xxxv. 383.

cident

Very dangerous explofion from the combination of half an ounce of phoſphorus, ten ounces of water and one ounce of ſulphur on a ſand heat.

cident alluded to, happened in the following manner: Half an ounce of phoſphorus, cut into pieces of the ſize of a pea, was introduced into a Florence flaſk, containing about ten ounces of water; one ounce of ſulphur broken into fragments of about the ſame ſize was added, and the whole placed on a heated ſand-bath. In a few minutes the union of the phoſphorus and ſulphur was effected. On leaving the whole in the heated ſand, for about ten minutes longer, the empty part of the flaſk became filled with denſe white fumes, which increaſed more and more; being unable to obſerve what change was taking place, I carefully removed the flaſk out of the ſand-bath, and agitated the fluid in ſuch a manner, that the fuſed compound of phoſphorus and ſulphur ſtill remained under the ſurface of the water. But the inſtant this was done, the whole exploded in my hand with a tremenduous report; the mixture of the burning phoſphorus was thrown into my face, and occaſioned very painful wounds; the pieces of the Florence flaſk were ſcattered all over the laboratory, as fine as ſand, and the larger parts of the neck of this veſſel were driven into my right hand, as well as into the wall, to a conſiderable depth.

Repetition of the experiment with ſmaller and alſo with larger quantities of the materials. Exploſion as before.

Anxious to underſtand the nature of this unexpected exploſion I again expoſed to heat, in a ſimilar manner, two drachms of phoſphorus and half an ounce of coarſely powdered ſulphur, in a ſmall flaſk containing four ounces of water. The mixture, after having been left in a heated ſand-bath for about ten minutes, exploded with prodigious violence, and a flaſh of fire roſe up to the ceiling. The ſame experiment was repeated with larger quantities of phoſphorus and ſulphur, three ſucceſſive times, with ſimilar effects. Theſe experiments were made at the laboratory, and in the preſence of the Right Honourable Lord Camelford, who liberally ſupplied the materials for theſe and the following experiments, and permitted them to be made on his premiſes.

Another accidental exploſion of the ſame nature.

Before I advance any thing further concerning the accenſion of this compound, I beg leave to relate one inſtance more of a ſimilar nature, which happened lately in my own laboratory. Mr. Garden, a philoſophical gentleman immerſed into a veſſel filled with warm water, a vial containing ſix ounces of phoſphorus, to which had been previouſly added one drachm of a mixture of phoſphorus and ſulphur. The contents of the

the vial being liquified, (which was the intent of immerfing it into heated water) he removed the vial out of the fluid, taking care to clofe its orifice with his finger, and then agitated it gently. The moment this was began the vial burft to pieces with a report like a gun, the burning mixture was thrown in all directions, and the whole laboratory was filled for fome hours with a very denfe cloud of white vapours.

Being thus fufficiently convinced of the danger which attends the combination of phofphorus with fulphur, under fuch circumftances, I introduced into a Wedgwood's tube clofed at one end, two drachms of phofphorus, and double that quantity of fulphur. I then added four ounces of water, and clofed the other extremity of the tube with a cork, into which a bended tube was cemented, which terminated under a glafs cylinder filled with mercury, ftanding inverted in a bafon containing the fame fluid. I then reclined the tube, and applyed heat to that part which contained the phofphorus and fulphur; on increafing the heat gradually a quantity of gas was collected, which amounted to nearly two quarts. But no explofion took place.

Careful examination of the phenomena. Phofphorus, fulphur and water expofed to diftillation.

afforded a gas without explofion.

To learn the nature of this gas, I transferred a quantity of it into the water apparatus, and agitated it in contact with that fluid ftrongly for a few minutes. Its volume was now confiderably diminifhed. On repeating the experiment in diftilled water, it was found that this fluid abforbed nearly $\frac{1}{4}$ of its own bulk. On fending up one part of atmofpheric air into a cylinder holding fix parts of this gas, an inftantaneous inflammation enfued, the cylinder became filled with white fumes, and a white cruft lined the inner furface of the glafs. Finding thus that the gafeous product was decompofable by atmofpheric air, I collected another quantity of gas, in a fimilar manner as before; and mingled it gradually with oxigen gas till no further accenfion enfued. The gas left behind amounted to $\frac{1}{64}$ of the whole. It had all the properties of nitrogen gas.

which, added to common air, diminifhed that fluid;

but took fire when a fmall portion of common air was added to a larger of the gas.

The refidue of this gas decompofed by oxigen was azote.

The white flakes which were collected from the fides of the glafs cylinder, as well as from the furface of the mercury over which the experiments were made, attracted moifture rapidly, and became converted into a cream-like fluid. They confifted of fulphur, fulphuric and phofphoric acids.

The precipitates were fulphur, fulphuric and phofphoric acids.

Oxigenized muriatic acid gas acted more violently than oxigen gas, when mingled with this gaseous compound over mercury; the result was a considerable detonation, accompanied with vivid green light and dense white vapours.

The gas was therefore a compound of hydrogen, sulphur, and phosphorus;

From the results of these experiments it becomes obvious, that the gas under examination was a compound of hydrogen, sulphur, and phosphorus. And if we reason from the nature of the production of this gas, it is evident, that, during the action of the phosphorus and sulphur upon water, the latter fluid is decomposed, though neither sulphur nor phosphorus singly taken, can effect this decomposition; this therefore is sufficient to account for the unexpected explosion before stated.

formed by the decomposition of water; though that fluid is not affected by either singly.

Phosphuret of sulphur can decompose water at the common temperature.

In order to see whether phosphuret of sulphur were capable of decomposing water in common temperatures, two ounces of it were covered in a vial with eight ounces of water, and put aside for further examination. The vial having been left unobserved, locked up in a closet, for some weeks; the corks was found to have been thrown out of the vial, and the whole inside of the closet, which had been painted with white lead, was completely blackened; the parts nearest to the orifice of the vial had a metallic aspect, The fluid which was decanted from the phosphuret of sulphur had a milky appearance, its odour was like that of water strongly impregnated with sulphurated hidrogen; its taste was uncommonly nauseous. It had a strong action on the greater number of metallic oxides.

On mingling it with concentrated nitrous acid, a considerable precipitate ensued, which after being dried on exposure to air, was luminous in the dark and became converted into sulphur. Another quantity of the water which was suffered to evaporate, spontaneously deposited crystals of a lemon yellow colour, but of an indeterminate figure. There remained, therefore, no doubt but that phosphuret of sulphur is capable of decomposing water at usual temperatures.

As phosphuret of sulphur decomposes air with greater rapidity than any other substance, it will be a good eudiometrical agent.

Phosphuret of sulphur, composed of three parts of phosphorus and one of sulphur, has also the property of decomposing atmospheric air with great rapidity. It may, therefore, be employed more advantageously for eudiometrical processes than either phosphorus, or the sulphurets of earths, alkalies, or metals. If into a *dry* glass tube closed at the top, and graduated

graduated into equi-diſtant parts, a quantity of phoſphuret of ſulphur, freed from adhering water, be poured, and agitated in the tube, ſo as to line a conſiderable part of it within, a vaſt quantity of white vapour is produced the moment the tube is immerſed in water; ſo as to exclude the air. The vapours will be abſorbed by the water, and when no further clouds appear, the proceſs is at an end. The reſiduary gas will then be found to be the quantity of nitrogen gas, which was contained in the air experimented upon in the tube. This proceſs is far more expeditious than the ſlow combuſtion of phoſphorus. I muſt, nevertheleſs, remark, that phoſphuret of ſulphur *like all other ſubſtances* hitherto employed for eudiometry, cannot be abſolutely depended upon for aſcertaining the abſolute quantity of oxigen, contained in a given portion of atmoſpheric air. For as ſoon as the abſorption of oxigen is compleated, the remaining nitrogen exerciſes an action upon the phoſphorus, by means of which its bulk becomes increaſed. From a number of experiments made with that view with this eudiometrical ſubſtance, I am led to believe that the volume of nitrogen gas, never increaſes ſo much as to $\frac{1}{3}$ part; conſequently the bulk of the reſiduum, diminiſhed by $\frac{1}{35}$ gives us the bulk of the nitrogen gas of the air examined; which bulk ſubtracted from the original maſs of air, indicates the proportion of oxigen gas contained in it.

This phoſphuret however, has the uſual imperfection of changing the reſidue.

The change is ſmall and may be allowed for.

Phoſphuret of ſulphur of the above compoſition alſo decompoſes nitrous acid with uncommon rapidity at common temperatures. If one part of phoſphuret of ſulphur be introduced in the cold into four or ſix of concentrated nitrous acid, a violent action takes place, the acid is decompoſed, and both the phoſphorus and ſulphur are oxigenized, at the expenſe of the oxigen of the nitric acid. A clear ſolution is obtained, from which phoſphoric and ſulphuric acid may be ſeparated in the uſual manner.

This phoſphuret decompoſes nitrous acid very rapidly.

Phoſphuret of ſulphur is ſoluble in expreſſed or fat oils. If one part of this compound, freed from adhering moiſture as much as poſſible, be triturated in a Wedgwood's mortar, with ſix parts of oil of almonds or olives, a liquid phoſphorus is obtained, which is far ſuperior to that produced in the uſual manner from mere phoſphorus. This liquid phoſphorus ſhines and gives a very fine liquid phoſphorus, with a beautiful yellow light. It may be rubbed over the face, hands, &c. without injury,

Phoſphuret of ſulphur is ſoluble in fat oils.

injury, provided the fluid be perfectly tranſparent, and conſequently contains not a particle of phoſphuret of ſulphur mechanically ſuſpended.

Which is uncommonly luminous.

The luminous property of this liquid phoſphorus is ſo conſiderable, that about 4oz. of it, when contained in a common ſize wine decanter, gives a ſufficient light to diſcern objects at a conſiderable diſtance in a large room, the moment the decanter is unſtopped.

Beautiful experiment of a luminous ſhower.

Equal parts of this liquid phoſphorus and oil of turpentine, when agitated together and poured out of any convenient perforated veſſel, exhibits a beautiful phenomenon, greatly reſembling a luminous rain, or ſhower of fire.

Caution againſt heat.

Though a liquid phoſphorus may be obtained, as directed before, no attempt ſhould be made to apply heat to a mixture of phoſphuret of ſulphur. For an exploſion was always the reſult whenever I attempted any proceſs of that kind.

Phoſphuret of ſulphur is ſoluble in ether.

Luminous phenomena.

Phoſphuret of ſulphur is ſoluble in ſulphuric and nitric ether. If either of theſe fluids be ſuffered to ſtand for ſome weeks, over a quantity of phoſphuret of ſulphur in a cloſed vial; part of the compound becomes diſſolved in the ether. If the ether be ſuffered to evaporate ſpontaneouſly, or when aſſiſted by heat, a multitude of exceedingly ſmall cryſtals are left behind, which ſhine in the dark with a brilliant yellow light. A piece of cloth dipped into this ether appears luminous in the dark all over, but in a few minutes this luminous appearance ceaſes, and the whole appears to be ſprinkled over with gems. If a feather, or piece of tow, be dipt in water, and then thrown into a bottle containing ether impregnated with phoſphuret of ſulphur; at the moment of contact of the two fluids, a ſudden light of a yellow colour ſpreads through the air, undulates along the ſurface of the fluid, and illuminates the whole bottle.

Phoſphuret of ſulphur in oil of turpentine, and other volatile oils.

Phoſphuret of ſulphur is ſoluble in pure or rectified oil of turpentine, oil of roſemary, oil of lavender, and in the reſt of the volatile oils met with in commerce. The ſolutions are all luminous in the dark, and depoſit the phoſphuret of ſulphur when ſlowly evaporated, in the form of needle-ſhaped cryſtals.

Sparingly in alcohol.

Highly rectified alcohol takes up a ſmall quantity of phoſphuret of ſulphur, the alcolic ſolution is decompoſable by the addition of water.

It takes fire in ox. mur. acid gas.

Phoſphuret of ſulphur takes fire ſpontaneouſly in oxigenized muriatic acid gas. If a ſmall quantity of dry phoſphuret of ſulphur

phur be introduced into a metallic ſpoon, and then immerſed in a bottle filled with oxigenized muriatic acid gas; the compound inſtantly kindles and burns with great vividneſs. The reſults of this experiment of courſe are phoſphoric, ſulphuric, and muriatic acids.

When already inflamed it continues to burn in nitrous gas and in the nitrous oxide.

Phoſphuret of ſulphur, when in a ſtate of inflammation burns alſo in nitrous gas, and in gaſeous oxide of nitrogen.

Combuſtion in the pneumatic vacuum is very advantageouſly performed on this compound.

If a piece of cotton be impregnated with phoſphuret of ſulphur, and then ſurrounded with wool or tow, and placed under the receiver of an air-pump, the compound ſhines with a beautiful yellow light, which increaſes in proportion as the air is more rarefied. On re-admitting a ſmall portion of air, a beautiful *Corona* or Aurora Borealis pervades the receiver. If a thermometer be included into the cotton containing the phoſphuret of ſulphur, it riſes in proportion as the light increaſes, and in this reſpect, as well as in the former, the phoſphuret of ſulphur anſwers better for this experiment, which was firſt noticed by Van Marum, who made uſe of phoſphorus.

No heat is developed when the combination of phoſphorus and ſulphur is made.

The formation of phoſphuret of ſulphur ſeems not to be attended with any change of temperature, as is ſaid to be always the caſe in all chemical combinations whatever. For if the phoſphorus and ſulphur be immerſed in heated water, at a diſtance from each other, together with a thermometer, no increaſe or decreaſe of temperature could be obſerved by means of the moſt delicate inſtrument.

It is more poiſonous than phoſphorus.

The compound of phoſphorus and ſulphur acts more violently in deſtroying animal life, than phoſphorus alone. A cat which had eaten two grains of phoſphorus repeatedly without impunity, died within half an hour after having ſwallowed one grain of phoſphuret of ſulphur.

Old Compton-Street, Soho,
Auguſt 16, 1803.

II. *Obſervations*

II.

(*Concluded from Page* 304. Vol. V)

Observations of the Tranſit of Mercury over the Diſk of the Sun; to which is added, an Inveſtigation of the Cauſes which often prevent the proper Action of Mirrors. By WILLIAM HERSCHEL, *LL. D. F. R. S.*

Mercury neatly defined.

WITH a 10-feet reflector, and magnifying power of 130, I ſaw the corrugations of the luminous ſolar ſurface, up to the very edge of the whole periphery of the diſk of Mercury.

10^h 27′. When the planet was ſufficiently advanced towards the largeſt opening of the northern zone, I compared the intenſity of the blackneſs of the two objects; and found the diſk of Mercury conſiderably darker, and of a more uniform black tint, than the area of the large opening.

10^h 32′. The preceding limb of Mercury cuts the luminous ſolar clouds with the moſt perfect ſharpneſs; whereas, in the great opening, the deſcending parapet, down the preceding ſide, was plainly viſible.

It ſhould be remarked, that the inſtrument here applied to the ſun, with the moderate power of 130, is the ſame 10-feet reflector which, in fine nights, when directed to very minute double ſtars, will ſhow them diſtinctly with a magnifier of 1000.

Great magnifying power not ſuitable to the ſun.

Having often attempted to uſe high magnifiers in viewing the ſun, I wiſhed to make another trial; though pretty well aſſured I ſhould not ſucceed, for reaſons which will appear hereafter.

With two ſmall double convex lenſes, both made of dark green glaſs, and one of them having the ſide which is neareſt the eye thinly ſmoked, in order to take off ſome light, I viewed the ſun. Their magnifying power was about 300; and I ſaw Mercury very well defined; but that complete diſtinctneſs, which enables us to judge with confidence of the condition of the object in view, was wanting.

With a ſingle eye-glaſs, ſmoked on the ſide towards the eye, and magnifying 460 times, I alſo ſaw Mercury pretty well defined; but here the ſun appeared ruddy, and no very minute objects could be perceived.

11^h

$11^h\ 28'$. The planet having advanced towards the preceding limb of the sun, it was now time to attend to the appearances of the interior and exterior contacts.

Observations of the contacts.

$11^h\ 32'$. 10-feet reflector. The whole disk of Mercury is as sharply defined as possible; there is not the least appearance of any atmospheric ring, or different tinge of light, visible about the planet.

Not the least sign of any atmosphere about Mercury.

$11^h\ 37'$. Appearances remain exactly as before.

$11^h\ 42'$. The sharp termination of the whole mercurial disk, appears to be even more striking than before. This may be owing to its contrast with the bright limb of the sun, which, having many luminous ridges in the northern zone, is remarkably brilliant about the place of the planet.

$11^h\ 44'$. I was a few moments longer writing down the above than I should have been, to see the interior contact so completely as I could have wished; however, the thread of light on the sun's limb was but just breaking, or broken; but no kind of distortion, either of the limb or of the disk of Mercury, took place.

At the interior contact, no distortion either of the sun's limb or the disk of Mercury,

The appearance of the planet, during the whole time of its emerging from the sun, remained well defined, to the very last.

nor during the emerging nor afterwards.

The following limb of Mercury remained sharp till it reached the very edge of the sun's disk; and vanished without occasioning the smallest distortion of the sun's limb, in going off, or suffering the least alteration in its own figure.

As soon as the planet had quitted the sun, the usual appearance of its limb was so instantly and perfectly restored, that not the least trace remained whereby the place of its disappearance could have been distinguished from any other adjacent part of the solar disk.

It will not be amiss to add, that very often, during the transit, I examined the appearance of Mercury with a view to its figure, but could not perceive the least deviation from a spherical form; so that, unless its polar axis should have happened to be situated, at the time of observation, in a line drawn from the eye to the sun, the planet cannot be materially flattened at its poles.

No signs of an oblate figure.

OBSERVATIONS AND EXPERIMENTS RELATING TO THE CAUSES WHICH OFTEN AFFECT MIRRORS, SO AS TO PREVENT THEIR SHOWING OBJECTS DISTINCTLY.

The action of reflecting telescopes is very different at different times.

It is well known to aftronomers, that telefcopes will act very differently at different times. The caufe of the many difappointments they may have met with in their obfervations, is however not fo well underftood.

Sometimes we have feen the failure afcribed to certain tremors, as belonging to fpecula; and remedies have been pointed out for preventing them. Not unfrequently again, the telefcope itfelf has been condemned; or, if its goodnefs could not admit of a doubt, the weather in general has been declared bad, though poffibly it might be as proper for diftinct vifion as any we can expect in this changeable climate.

The experience acquired by many years of obfervation, will however, I believe, enable me now to affign the principal caufe of the difappointments to which we are fo often expofed. Unwilling to hazard any opinion that is not properly fupported by facts, I fhall have recourfe to a collection of occafional obfervations. They have been made with fpecula of undoubted goodnefs, fo that every caufe which impeded their proper action muft be looked upon as extrinfic. I fhall arrange thefe obfervations under different heads, that, when they have been related, there may remain no difficulty to draw a few general conclufions from them, which will be found to throw a confiderable light upon our fubject.

Moifture in the Air.

Whether moifture in the air impedes the action of telefcopes.

(1.) October 5, 1781. I fee double ftars, with 460, completely well. The air is very damp.

(2.) Nov. 23, 1781. 15^h 30$'$. The morning is uncommonly favourable, and I fee the treble ftar ζ Cancri, with 460, in high perfection. The air is very moift, and intermixed with paffing clouds.

(3.) Sept. 7, 1782. I viewed the double ftar preceding 12 Camelopardalis,* with 932. In this, and feveral other fine nights which I have lately had, the condenfing moifture on the tube of my telefcope has been running down in ftreams; which proves that damp air is no enemy to good vifion.

* See Phil. Tranf. Vol. LXXV. Part I. page 68; II. 53.

(4.) Dec.

(4.) Dec. 28, 1782. $17^h\ 30'$. The water condenſing on my tube keeps running down; yet I have ſeen very well all night. I was obliged to wipe the object-glaſs of my finder almoſt continually. The ſpecula, however, are not in the leaſt affected with the damp. The ground was ſo wet that, in the morning, ſeveral people believed there had been much rain in the night, and were ſurpriſed when I aſſured them there had not been a drop.

(5.) Feb. 19, 1783. I have ſeen perfectly well till now * that a froſt is coming on; though Datchet Common, which is juſt before my garden, is all under water; and the graſs on which I ſtand with my teleſcope is as wet as poſſible.

(6.) Feb. 26, 1783. All the ground is covered with ſnow; yet I ſee remarkable well.

(7.) March 8, 1783. The common before my garden is all under water; my teleſcope is running with condenſed vapour; not a breath of air ſtirring. I never ſaw better.

(8.) Auguſt 25, 1783. My teleſcope ran with water all the night. The ſmall ſpeculum, which ſometimes gathers moiſture, was never affected in the 7-feet tube, but was a little ſo in the 20-feet. The large eye-glaſſes and object-glaſſes of the finders, required wiping very often. I ſaw all night remarkably well.

Fogs.

Whether fogs impair the diſtinctneſs of teleſcopes.

(9.) Oct. 30, 1779, It grows very foggy, and the moon is ſurrounded with ſtrong nebuloſity; nevertheleſs, the ſtars are very diſtinct, and the teleſcope will bear a conſiderable power.

(10.) Auguſt 20, 1781. It is ſo foggy that I cannot ſee an object at the diſtance of 40 feet; yet the ſtars are very diſtinct in the teleſcope. By an increaſe of the fog, α Piſcium can no longer be ſeen by the eye; yet, in the teleſcope, it being double, I ſee both the ſtars with perfect diſtinctneſs.

(11.) Sept. 6, 1781. A fog is come on; yet I ſee very well.

(12.) Sept. 9, 1781. There is ſo ſtrong a fog, that hardly a ſtar leſs than 30° high is to be ſeen; and yet, in the teleſcope, at great elevations, I ſee extremely well.

* The time is not marked in the journal; but, from the number of the obſervations that had been made during the night, it muſt have been towards morning.

(13.) March

(13.) March 9, 1783. It is very foggy; yet in the telescope I see the stars without aberration, and they are very bright. α Serpentarii is without a single ray.

(14.) April 6, 1783. A very thick fog settles upon all my glasses; but the specula, even the 20-feet, which has so large a surface, remained untouched. I see perfectly well.

Frost.

Whether frost be an impediment to distinct vision by telescopes.

(15.) Nov. 15, 1780; five o'clock in the morning. An excellent speculum, No. 2, will not act properly; the frosty morning probably occasions an alteration in its figure. Another speculum, No. 1, acts but indifferently, though I have known it to shew very well formerly in a very hard frost: for instance, November 23, 1779, I saw with the same mirror, and a power of 460, the vacancy between the two stars of the double star Castor, without the least aberration.

(16.) Oct. 22, 1781. Frost seems to be no hindrance to perfect vision. The tube of my 7-feet telescope is covered with ice; yet I see very well.

(17.) Nov. 19, 1781. It freezes very hard, and the stars, even those which are 50° high, are very tremulous. I suspect their apparent diameters to be diminished; and, if I recollect right, this is not the first time that such a suspicion has occurred to me.

(18.) Jan. 10, 1782. My telescope would not act well, even at an altitude of 70 or 80 degrees. There is a strong frost.

(19.) Jan. 31, 1782. I cannot see with a power of 460, the stars seem to dance so unaccountably, and yet the air is perfectly calm: even at 60 or 70 degrees of altitude, vision is impaired.

(20.) Feb. 9, 1782. That frost is no hindrance to seeing well is evident; for, not only my breath freezes upon the side of the tube, but more than once have I found my feet fastened to the ground, when I have looked long at the same star.

(21.) Oct. 4, 1782. It froze very severely this night. At first, when the frost came on, I saw very badly, every object being tremulous; but, after some time, and at proper altitudes, I saw as well as ever. Between five and six o'clock in the morning, objects began to be tremulous again; occasioned, I suppose, by the coming on of a thaw.

(22.) Jan.

(22.) Jan. 1, 1783. I made a number of delicate obſervations this night, notwithſtanding, at four o'clock in the morning, my ink was frozen in the room; and, at about five o'clock, a 20-feet ſpeculum, in the tube, went off with a crack, and broke into two pieces. On looking at Fahrenheit's thermometer, I found it to ſtand at 11°.

(23.) May 6, 1783. It freezes, and in the teleſcope the ſtars ſeem to dance extremely.

Hoar-froſt.

(24.) Nov. 6, 1782. There is a thick hoar-froſt; yet I ſee extremely well. It ſeems to enlarge the diameters of the ſtars; but, as I ſee the minuteſt double ſtars well, the apparent enlargement of the diameters muſt be a deception. Hoar froſt; its effect on teleſcopes.

(25.) Dec. 22, 1782. There is a ſtrong hoar-froſt gathering upon the tubes of my teleſcopes; but I ſee very well.

Dry Air.

(26.) Dec. 21, 1782. The tube of my teleſcope is dry, and I do not ſee well. Dry air inimical to diſtinct viſion by teleſcopes.

(27.) April 30, 1783. The ſtars are extremely tremulous and confuſed; the outſide of the tube of my teleſcope is quite dry.

Northern Lights.

(28.) Sept. 25, 1781. There are very ſtrong northern lights; their flaſhing does not ſeem to interfere with teleſcopic viſion; but all objects appear tremulous, and indifferently defined. Northern lights do not ſeem to impede viſion by teleſcopes.

(29.) Aug. 30, 1782. There are very bright northern lights, in broad arches, with white ſtreaks; yet I ſee perfectly well.

(30.) March 26, 1783. An Aurora Borealis is ſo bright, that η Herculis, which it covers, can hardly be ſeen; yet, in the teleſcope, and with a power of 460, I find no difference. I compared the ſtar with γ Coronæ, which was in a bright part of the heavens, and in the teleſcope they appeared nearly alike. I ſuſpected η Herculis to be ſomewhat more tinged with red than it ſhould be; and examined it afterwards, when clear of the Aurora: it was indeed leſs red; but, as it had gained more altitude, the experiment was not deciſive.

Windy

Windy Weather.

Wind impairs the effect of teleſcopes.

(31.) Jan. 8, 1783. It is very windy. The diameters of the ſtars are ſtrangely increaſed, even thoſe at 60 and 70° of altitude. Every ſtar ſeems to be a little planet.

(32.) Jan. 9, 1783. Wind increaſes the apparent diameters of the ſtars.

(33.) Sept. 20, 1783. The night has been very windy; and I do not remember ever to have ſeen ſo ill, with ſuch a beautiful appearance of brilliant ſtar-light.

Fine in Appearance.

Weather apparently fine but unfavourable.

(34.) May 28, 1781. The evening, though fine in appearance, is not favourable. No inſtrument I have will act properly. The wind is in the eaſt.

(35.) Auguſt 30, 1781. The ſtars appear fine to the naked eye, ſo that I can ſee ϵ Lyræ very diſtinctly to be two ſtars; yet my teleſcope will ſhow nothing well. There are flying clouds, which, by their rapid motion, indicate a diſturbance in the upper regions of the air; though, excepting now and then a few guſts of wind, it is in general very calm. At a diſtance there are continual flaſhes of lightning, but I can hardly hear any thunder.

(36.) Sept. 14, 1781. I ſee very ſmall ſtars with the naked eye; but the teleſcope will not act ſo well as it ſhould.

(37.) Sept. 24, 1781. The evening is apparently fine; but, with the teleſcope, I can ſee neither η Coronæ nor μ Bootis double; nor indeed can I ſee any other ſtars well.

Over a Building.

Vicinity of a building renders the ſtars indiſtinct;

(38.) Auguſt 24, 1783. I viewed ϵ Bootis with 449,737, and 910, but ſaw it very indifferently. The ſtar was over a houſe.

(39.) Oct. 26, 1780. ϵ Bootis being near the roof of a houſe, I ſaw it not ſo diſtinctly as I could wiſh.

The Teleſcope lately brought out.

Recent expoſure of the teleſcope does not afford diſtinctneſs.

(40.) Oct. 10, 1780. $6^h\ 30'$. Having but juſt brought out my teleſcope, it will not act well.

$6^h\ 45'$. The tube and ſpecula are now in order, and perform very well.

(41.) Jan. 11, 1782. To all appearance, the morning was very fine, but ftill the telefcope, when firft brought out, would not act well. After half an hour's expofure, it performed better.

Confined Place.

(42.) July 19, 1781. 13^h 15′. My telefcope would not act well; and, fuppofing the exhalations from the grafs in my garden to affect vifion, I carried the telefcope into the ftreet, (the obfervation was made at Bath,) and found it to perform to admiration.

A confined place prevents telefcopes from acting well.

(43.) July 19, 1781. My telefcope acted very well; but a flight field-breeze fpringing up, and brufhing through the ftreet where my inftrument was placed, it would no longer bear a magnifying power of 460.

Hazinefs and Clouds.

(44.) Sept. 22, 1783. The weather is now fo hazy, that the double ftar δ Cygni is but barely vifible to the naked eye. This has taken off the rays of the large ftar, fo that I now fee the fmall one extremely well, which at other times it is fo difficult to perceive, even with a magnifying power of 932.

Remarkable effect of hazinefs and clouds.

(45.) Auguft 13, 1781. A cloud coming on very gradually upon fixed ftars, has this remarkable effect, that their apparent diameters diminifh gradually to nothing.

(46.) July 7, 1780. The air was very hazy, but extremely calm. I had Arcturus in the field of view of the telefcope, and, the hazinefs increafing, it had a very beautiful effect on the apparent diameter of this ftar, For, fuppofing the firft of the points *, to reprefent the magnitude when brighteft, I faw it gradually decreafe, and affume, with equal diftinctnefs, the form of all the fucceeding points, from No. 1 to No. 10, in the order of the numbers placed over them. The laft magnitude I faw it under, could certainly not exceed two-tenths of a fecond; but was perhaps lefs than one. This leads to the difcovery of one of the caufes of the apparent magnitude of the fixt ftars.

One caufe of the apparent diameters of the fixed ftars.

* Thefe points will be inferted in one of the plates in our next number. N.

Focal

Focal Length.

Obfervations in which the focal length and figure of the fpeculum was altered by folar heat.

(47.) Nov. 14, 1801. The focal length of my 10-feet mirror increafes by the heat of the fun. I have often obferved this before; the difference, by feveral trials, amounts to 8 hundredths of an inch.

(48.) Dec. 13, 1801. The focal length of my 10-feet mirror, while I was looking at the fun, became fhorter, contrary to what it ufed to do; but, there being a ftrong froft, I guefs that the object metal grows colder, notwithftanding its expofure to the fun's rays.

(49.) Nov. 9, 1802. $10^h\ 50'$. The focus of my 7-feet glafs mirror became 18 hundredths of an inch fhorter, on being expofed for about a minute to the fun. The figure of the fpeculum was alfo diftorted; the foci of the infide and outfide rays differing confiderably, though its curvature, by obfervations on the ftars, has been afcertained to be ftrictly parabolical.

$12^h\ 0'$. The fame mirror, expofed one minute to the action of the fun, became 21 hundredths fhorter in focal length.

The focus of a 10-feet metalline mirror, when expofed one minute to the fun's rays, became 15 hundredths of an inch longer than it was before.

(50.) January 9, 1803. When I looked with the glafs 7-feet mirror, feveral times, a minute or two at the fun, it fhortened generally ,24, ,26, and ,30 of an inch, in focal length.

The obfervations which are now before us, appear to be fufficient to eftablifh the following principle; namely,

General principle. *Uniform temperatures and moifture in the air are requifite to diftinct vifion.*

" That in order to fee well with telefcopes, it is required that the temperature of the atmofphere and mirror fhould be uniform, and the air fraught with moifture."

This being admitted, we fhall find no difficulty in accounting for every one of the foregoing obfervations.

This doctrine applied to, fudden changes of temperature;

If an uniform temperature be neceffary, a froft after mild weather, or a thaw after froft, will derange the performance of our mirrors, till either the froft or the mild weather are fufficiently fettled, that the temperature of the mirror may accommodate itfelf to that of the air. For, till fuch an uniformity with the open air, in the temperature of the mirror, the tube, the eye-glaffes, and I would almoft add the obferver, be obtained, we cannot expect to fee well. See obfervation 15, 17, 18, 19, and 23.

But

But, when a frost, though very severe, becomes settled, the mirror will soon accommodate itself to the temperature; and we shall find our telescopes to act well. See observation 16, 20, 21, 22, 24, and 25.

or of exposure;

This explains, with equal facility, why no telescope just brought out of a warm room can act properly. See observation 40 and 41.

observations from a confined place;

Nor can we ever expect to make a delicate observation, with high magnifying powers, when looking through a door, window, or slit in the roof of an observatory; even a confined place, though in the open air, will be detrimental. See observation 42 and 43.

windy weather;

It equally shows, that windy weather in general, which must occasion a mixture of airs of different temperatures, cannot be favourable to distinct vision. See observation 31, 32, and 33.

auroræ boreales;

The same remark will apply to Auroræ Boreales, when they induce, as they often do, a considerable change in the temperature of the different regions of air. See observation 28.

But, should they not be accompanied by such a change, there seems to be no reason why they should injure vision. See observation 29 and 30.

the roof of an house;

The warm exhalations from the roof of a house in a cold night, must disturb the uniformity of the temperature of a small portion of air; so that stars which are over the house, and at no considerable distance, may be affected by it. See observation 38 and 39.

weather apparently fine;

Sometimes the weather appears to be fine, and yet our telescopes will not act well. This may be owing to dryness occasioned by an easterly wind; or to a change of temperature, arising from an agitation of the upper regions of the atmosphere. See observation 34 and 35.

Or, possibly, to both these causes combined together. See observation 36 and 37.

If moisture in the atmosphere be necessary, dry air cannot be proper for vision. See observation 26 and 27.

damps, haze;

And therefore, on the contrary, dampness, and haziness of the atmosphere, must be favourable to distinct vision. See observation 1, 2, 3, 4, 6, and 8.

fogs, &c.

Fogs also, which certainly denote abundance of moisture, must be very favourable to distinct vision. See observation 9, 10, 11, 12, 13, and 14.

Nay, if the obſervatory ſhould be ſurrounded by water, we need be under no apprehenſion on that account. Perhaps, were we to erect a building for aſtronomical purpoſes only, we ought not to object to grounds which are occaſionally flooded; the neighbourhood of a river, a lake, or other generally called damp ſituations. See obſervation 5 and 7.

It is however poſſible, that fogs and hazineſs may increaſe to ſuch a degree as, at laſt, to take away, by their interpoſition, all the light which comes from celeſtial objects; in which caſe, they muſt of courſe put an end to obſervation; but they will nevertheleſs be accompanied with diſtinct viſion to the very laſt. See obſervation 44, 45, and 46.

We have now only the four laſt obſervations to account for. They relate to the change of the focal length of mirrors in ſolar obſervations, and its attendant derangement of the foci of the different parts of the reflecting ſurface; and, as ſimplicity is one of the marks of the truth of a principle, I believe we need not have recourſe to any other cauſe than the change of temperature produced by the action of the ſolar rays that occaſion heat; which will be quite ſufficient to explain all the phenomena. But, in order to ſhow this in its proper light, I ſhall relate the following experiments.

1ſt Experiment.

Experiments on the change of focal length in mirrors by heat.

I placed a glaſs mirror, of 7-feet focal length, in the tube belonging to the teleſcope; and, having laid it open at the back, I prepared a ſtand, on which the iron uſed in my experiments on the terreſtrial rays that occaſion heat (ſee Phil. Tranſ. for 1800, Plate XVI. Fig. 1.) might be placed, ſo as to heat the mirror from behind, while I kept a certain object in the field of view of the teleſcope. Having meaſured the focal length, and alſo examined the figure of the mirror, which was parabolical, the heated iron was applied ſo as to be about 2¼ inches from the back of the glaſs mirror. The conſequence of this was, that a total confuſion in all the foci took place, ſo that the letters on a printed card in view, which before had been extremely diſtinct, became inſtantly illegible. In 15 ſeconds, the focus of the mirror was ſhortened 2,3 inches; in half a minute, 3,47 inches; and, at the end of the minute, I found it no leſs than 4,59 inches ſhorter than it had been before the application of the hot iron.

On

On repeating the experiment, but placing the heated iron no more than $\frac{1}{4}$ of an inch from the back of the mirror, its focal length, in $1\frac{1}{2}$ minute, became 5,33 inches ſhorter. Experiments on the change of focal length in mirrors by heat.

I tried alſo a more moderate heat; and, placing the iron at 3 inches from the back, the focus of the mirror ſhortened in one minute 2,83 inches.

A thermometer placed in contact with the reflecting ſurface of the mirror, could hardly be perceived to have riſen, during the time in which the hot iron produced the alteration of the focal length.

2d Experiment.

Every thing remaining as before, I ſuſpended a ſmall globe of heated iron in front of the mirror, at one inch and a half from its vertex; and, in two minutes, the focus was lengthened 5,3 inches. The figure of the mirror was alſo deranged; ſo that the letters on the card could not be diſtinguiſhed.

I made a ſecond trial, with the ſuſpended iron a little more heated, and brought it as near the ſurface of the mirror as I judged it to be ſafe; ſince a contact would probably have cracked the mirror. In conſequence of this arrangement, the focus lengthened, in one minute, 1,64 inch.

On removing the heated iron, the mirror returned, in one minute, to within ,18 inch of its former focal length; and, at the end of the ſecond minute ſeemed to be nearly reſtored. But the diſagreement of the foci of the different parts of the reflecting ſurface might be perceived for a long time afterwards, and cauſed an indiſtinctneſs of viſion, which plainly indicated that, under ſuch circumſtances, the magnifying power of the teleſcope, 225, was more than it ought to be, in order to ſee well.

3d Experiment.

I now changed the glaſs mirror for a metalline one; and, on placing the heater near the back of it, the focus of the ſpeculum, in 30 ſeconds, became ,77 inch ſhorter. But, continuing the obſervation, inſtead of ſhortening ſtill farther in the next 30 ſeconds, it became ,3 inch longer, ſo that, at the end of a minute, it was only ,47 ſhorter than before the approach of the hot iron.

C 2 4th Expe-

4th Experiment.

Experiments on the change of focal length in mirrors by heat.

When the ſmall heated globe of the 2d experiment was ſuſpended in front of the mirror, the focus lengthened ,27 inch in one minute; nor would the lengthening increaſe by leaving the hot iron longer in its poſition. The foci in this, as well as in the 3d experiment, were ſo much injured that they could not be meaſured with any preciſion; and it was evident, that high magnifying powers ought not to be uſed with a mirror of which the temperature is undergoing a continual change.

I repeated the experiment with the iron nearly red hot; and found the focus lengthened 1,48 inch in 30 ſeconds. Five minutes after the removal of the iron, the regularity of the figure of the mirror was pretty well reſtored.

With a moderate heat, I had, in 30 ſeconds, a lengthening of the focus, of ,57 inch; and, in about 1½ minute after the removal of the heated iron, diſtinct viſion was nearly reſtored.

Theſe four experiments ſhow, that a change in the temperature of mirrors, occaſioned by heat, is attended with an alteration of their focal length; and alſo prove, that the figure of the reflecting ſurface is conſiderably injured, during the time that ſuch a change takes place. We are conſequently authoriſed to believe, that the ſmall alteration in the focus of a mirror expoſed to the rays of the ſun, ariſes from the ſame cauſe. For, ſince a thermometer, when the ſun is ſhining upon it, will ſhow that its temperature is altered, the action of the ſolar rays upon a mirror muſt be attended with a ſimilar effect in its temperature. See obſervation 47, 48, 49, and 50.

The ſame experiments will now alſo explain why the obſervations of the ſun, related in our tranſit of Mercury, between $10^h\ 32'$ and $11^h\ 28'$, were not attended with ſucceſs; for we have ſeen that heat occaſions a derangement in the action of the reflecting ſurface; and it follows that, under ſuch circumſtances, high magnifying powers cannot be expected to ſhow objects very diſtinctly.

If it ſhould be remarked, that I have not explained why the focus of a glaſs mirror ſhould ſhorten by the ſame rays of the ſun which lengthen that of a metalline ſpeculum, I confeſs that this at preſent does not appear; and, as it is not material to our purpoſe, I might paſs it over in ſilence. We are however

pretty well assured, that the alterations of the focal length must be owing to a dilatation of the glass or metal of which mirrors are made, and must be greatest where most heat is applied. Our experiments therefore cannot agree perfectly with solar observations; for, in the glass mirror, the application of partial heat in front, must undoubtedly have been much stronger about the middle of the mirror (though the centre of it was sometimes guarded by a brass plate equal to the size of the small speculum) than at the circumference. But when, on the contrary, a mirror is exposed to the sun, every part of the surface will receive an equal portion of heat.

Experiments on the change of focal length in mirrors by heat.

It may also be said, that I have pointed out a defect in telescopes used for solar observations, without assigning a cure for it. It will however be allowed, that tracing an evil to its cause must be the first step towards a remedy. Had the imperfection of the figure brought on by the heat of the solar rays been of a regular nature, an elliptical speculum might have been used to counteract the assumed hyperbolical form; or *vice versâ*.

And now, as, properly speaking, the derangement of the figure of a mirror used in observing the sun, is not so much caused by the heat of its rays as by their partial application to the reflecting surface only, which produces a greater dilatation in front than at the back, there may be a possibility of counteracting this effect, by a contrary application of heat against the back, or by an interception of it on the front. But this we leave to future experiments.

III.

Observations on the Chemical Nature of the Humours of the Eye. *By* RICHARD CHENEVIX, *Esq. F.R.S. and M.R.I.A.**

THE functions of the eye, so far as they are physical, have been found subject to the common laws of optics. It cannot be expected that chemistry should clear up such obscure points of physiology, as all the operations of vision appear to be; but, some acquaintance with the intimate nature of the substances

* From the Philosophical Transactions, 1803.

which

which produce the effects, cannot fail to be a uſeful appendage to a knowledge of the mechanical ſtructure of the organ.

Humours of the eye little known chemically. Aqueous humour.

The chemical hiſtory of the humours of the eye, is not of much extent. The aqueous humour had been examined by Bertrandi; who ſaid, that its ſpecific gravity was 975, and therefore leſs than that of diſtilled water. Fourcroy, in his *Syſtème des Connoissances chimiques*, tells us, that it has a ſaltiſh taſte; that it evaporates without leaving a reſiduum; but that it contains ſome animal matter, with ſome alkaline phoſphate and muriate. Theſe contradictions only prove, that we have no accurate knowledge upon the ſubject.

Vitreous humour.

The vitreous humour is not better known. Wintringham has given its ſpecific gravity (taking water at 10000) as equal to 10024; but I am not acquainted with any experiments to inveſtigate its chemical nature.

Cryſtalline lens.

We are told by Chrouet, that the cryſtalline lens affords, by deſtructive diſtillation, fetid oil, carbonate of ammonia, and water, leaving ſome carbon in the retort. But, deſtructive diſtillation, although it has given us much knowledge as to animal matter in general, is too vague a method for inveſtigating particular animal ſubſtances.

I ſhall now proceed to mention the experiments I have made upon all the humours. I ſhall firſt relate thoſe which were made upon the eyes of ſheep, (they being the moſt eaſily procured,) and ſhall afterwards ſpeak of thoſe of the human body, and other eyes. I think it right to obſerve, that all theſe eyes were as freſh as they could be obtained.

SHEEPS' EYES.

Aqueous Humour.

Of ſheeps' eyes; The aqueous humour. Water, albumen, gelatine and muriate of ſoda.

The aqueous humour is a clear tranſparent liquid, of the ſpecific gravity of 10090 *, at 60° of Fahrenheit. When freſh, it has very little ſmell, or taſte.

It cauſes very little change in the vegetable reactive colours; and this little would not, I believe, be produced immediately after death. I imagine it to be owing to a generation of ammonia, ſome traces of which I diſcovered.

* All theſe ſpecific gravities are mean proportionals of ſeveral experiments. The eyes of the ſame ſpecies of animal, do not differ much in the ſpecific gravity of their humours.

When

When expofed to the air, at a moderate temperature, it evaporates flowly, and becomes flightly putrid.

When made to boil, a coagulum is formed, but fo fmall as hardly to be perceptible. Evaporated to drynefs, a refiduum remains, weighing not more than 8 per cent. of the original liquor.

Tannin caufes a precipitate in the frefh aqueous humour, both before and after it has been boiled, and confequently fhows the prefence of gelatine.

Nitrate of filver caufes a precipitate, which is muriate of filver. No metallic falts, except thofe of filver, alter the aqueous humour.

From thefe and other experiments it appears, that the aqueous humour is compofed of water, albumen, gelatine, and a muriate, the bafis of which I found to be foda.

I have omitted fpeaking of the action of the acids, of the alkalis, of alcohol, and of other re-agents, upon this humour. It is fuch as may be expected in a folution of albumen, of gelatine, and of muriate of foda.

Cryftalline Humour.

To follow the order of their fituation, the next of the humours is the cryftalline. **Cryftalline humour. Much larger proportion of albumen and gelatine.**

This differs very materially from the others.

Its fpecific gravity is 11000.

When frefh, it is neither acid nor alkaline. It putrifies very rapidly. It is nearly all foluble in cold water, but is partly coagulated by heat. Tannin gives a very abundant precipitate; but I could not perceive any traces of muriatic acid, when I had obtained the cryftalline quite free from the other humours. It is compofed, therefore, of a fmaller proportion of water than the others, but of a much larger proportion of albumen and gelatine.

Vitreous Humour.

I preffed the vitreous humour through a rag, in order to free it from its capfules; and, in that ftate, by all the experiments I could make upon it, I could not perceive any difference between it and the aqueous humour, either in its fpecific gravity, (which I have found to be 10090, like that of the other), or in its chemical nature. **Vitreous humour. The fame as the aqueous.**

M. Fourcroy

No phosphate in these humours.

M. Fourcroy mentions a phosphate, as contained in these humours; but I could not perceive any precipitation by muriate or nitrate of lime; nor did the alkalis denote the presence of any earth, notwithstanding M. Fourcroy's assertion of that fact.

HUMAN EYE.

Human eye; not chemically different from other eyes.

I could not procure a sufficient quantity of these, fresh enough to multiply my experiments upon them. However, by the assistance of Mr. Carpue, Surgeon to his Majesty's Forces, I fully convinced myself, that the humours of the human eye, chemically considered, did not contain any thing different from the respective humours of the eyes I had examined. The aqueous and vitreous humours contained water, albumen, gelatine, and muriate of soda; and the crystalline humour contained only water, albumen, and gelatine. The specific gravity of the aqueous and vitreous humours, I found to be 10053; while that of the crystalline was 10790.

EYES OF OXEN.

So likewise the eyes of oxen.

I found the eyes of oxen to contain the same substances as the respective humours of other eyes. The specific gravity of the aqueous and vitreous humours is 10088; and that of the crystalline 10765.

Probable law; that the smaller the eye the more does the density of the crystalline differ from that of the other humours.

What is particularly worthy of notice is, that the difference which appears to exist between the specific gravity of the aqueous or vitreous humour and that of the crystalline, is much greater in the human eye than in that of sheep, and less in the eye of the ox. Hence it would appear, that the difference between the density of the aqueous and vitreous humour and that of the crystalline, is in the inverse ratio of the diameter of the eye, taken from the cornea to the optic nerve. Should further experiments show this to be a universal law in nature, it will not be possible to deny that it is in some degree designed for the purpose of promoting distinct vision.

The crystalline is considerably more dense in approaching the centre.

In taking the specific gravity of the aqueous and vitreous humours, no particular precaution is necessary, except that they ought to be as fresh as possible. But the crystalline humour is not of an uniform density throughout; it is therefore essential, that attention be given to preserve that humour entire for this operation. I found the weight of a very fresh crystalline of an

ox

ox to be 30 grains; and its ſpecific gravity was, as I before ſtated, 10765, I then pared away all the external part, in every direction, till there remained but ſix grains of the centre, and the ſpecific gravity of theſe ſix grains, I found to be 11940. From this it would ſeem, that the denſity increaſes gradually, from the circumference to the centre.

It is not ſurpriſing that the cryſtalline humour ſhould be ſubject to diſorders, it being wholly compoſed of animal matter of the moſt periſhable kind. Fourcroy ſays, that it is ſometimes found oſſeous in advanced age. Albumen is coagulated by many methods; and, if we ſuppoſe that the ſame changes can take place in the living eye as in the dead animal matter of the chemiſts, it will be eaſy to account for the formation of the cataract; a diſorder which cannot be cured but by the removal of the opaque lens. If a ſufficient number of obſervations were made reſpecting the frequency of the cataract in gouty habits, ſome important concluſions might be drawn, as to the influence of phoſphoric acid, in cauſing the diſorder, by the common effect of acids, in coagulating albumen.

It is very ſubject to diſorders from its coagulable nature.

IV.

A Letter from Mr. Irvine *concerning the late Dr.* Irvine, *of Glaſgow, his Doctrine, which aſcribes the Diſappearance of Heat, without Increaſe of Temperature, to a change of Capacity in Bodies, and that of Dr.* Black, *which ſuppoſes Caloric to become latent by Chemical Combination with Bodies; with particular Remarks on the Miſtakes of Dr.* Thompson, *in his Accounts of theſe Doctrines.*

To Mr. NICHOLSON.

SIR,

IN the article Chemiſtry in the Supplement to the Encyclopœdia Britannica, in moſt reſpects excellently written, I could not fail to be ſtruck with the account there given of the theory of heat, and the mode of inveſtigating the natural zero adopted by the late Dr. Irvine. Had Dr. Thompſon been indiſputably accurate in *his* opinions, had a mathematically cloſe argument left no door for the entrance of doubt, he would ſcarcely even then

Account given in the Encycl. Britt. of the inveſtigation of the zero of heat.

then have been juftified in the terms he has ufed. But here we have every thing fettled, and we are informed with the air of an ancient fophift, that it is examined and found infufficient. I am induced to make an obfervation or two on this examination, becaufe I am informed that the mathematical air which reigns through this part of this work has actually impofed on many. For this time, however, I believe it can be fhewn that it is no more than an air, and that this fubject is not yet finally fettled.

Dr. Thompfon's ftatement of Dr. Irvine's theory of heat. That caloric abforbed by ice on its converfion into water (without increafe of temperature) only *feems to become latent* and is really *employed in keeping up the temperature while the capacity is increafed*; but that this does not explain the act of fufion. Argument of Dr. Thompfon. The capacity of water is to that of ice as 10 : 9.—Ice during fufion abforbs what would have raifed water 140°—it would therefore have raifed the ice at leaft as much if it had remained folid—whence it is inferred that the fufed ice or water ought to have been raifed 9—10ths or 126°, if the difference of capacity had alone operated in the cafe:

Dr. Thompfon fays, at page 269, Suppl. Ency. Brit. "Dr. Irvine, of Glafgow, advanced a theory on this fubject different from that of Dr. Black. The fpecific caloric of water being greater than that of ice, it requires a greater quantity of caloric to raife it to a given temperature than it does to raife ice. The caloric therefore does not become latent, it only feems to do fo from the greater fpecific caloric of water. This theory was zealoufly adopted by Dr. Crawford. Dr. Black obferved very juftly, that it did not account for the production of fluidity at all. The fpecific caloric of water is indeed greater than that of ice; but how is the ice converted into water? This is an objection which the advocates for Dr. Irvine's or Dr. Crawford's theory, (as it has been improperly called) will not eafily anfwer. Let us examine whether this theory accounts for the apparent lofs of caloric. It follows from Mr. Kirwan's experiments, that the fpecific caloric of water is to that of ice as 10 to 9. Dr. Black proved, that as much caloric entered the ice as would have raifed it had it been water, 140°. Let us fuppofe that it would only have raifed the ice 140; in that cafe the melted ice ought to have been of the temperature of 158°, for 10 : 9 : : 140 : 126, but it was only 32°. Therefore 126° of caloric have difappeared, and cannot be accounted for by the change of fpecific caloric. Nor can the accuracy of Dr. Black's experiment be fufpected; it has been repeated in every part of the world, and varied in every poffible way. We cannot doubt, therefore, that caloric unites with fubftances, and caufes them to become fluid, or that there is in fact a caloric of fluidity different from fpecific caloric."

Now nobody doubts Dr. Black's experiment, and it is not neceffary to our argument to have any doubt on that fubject. Dr. Thompfon gives it as a fair ftatement of Dr. Irvine's theory,

ory, that the 140° entering ice during its change of forms, should be leſſened in the ratio of 9 : 10, and ſhould therefore be 126°, or in other words, if the heat entering the ice were only enough to raiſe the ice 140°, that the temperature of the water ſhould be 158°. But it is eaſy to ſee that on this ſuppoſition the water would contain two portions of heat, namely, its original quantity from the natural zero to 32°, expreſſed in degrees according to the capacity of ice, and the ſuperadded portion from 32 to 158, expreſſed in degrees according to that of water. The ice would no doubt according to this ſtatement, become water without abſorbing any heat, and the water would be merely heated as in every other caſe. But Dr. Irvine, and after him Dr. Crawford, and the writers of elementary treatiſes for the laſt fifteen years, and I believe I may venture to ſay every one of our philoſophers, except Dr. Thompſon himſelf, have ſtated this doctrine of capacities to be, that every one of the thermometrical degrees expreſſing the whole of the heat contained in a body, are to be taken in proportion to its capacity; and therefore that if the ice ſuddenly changed its capacity, it would abſorb not merely a rateable proportion of what heat might be preſented to it, but an abſolute quantity to make up for its new capacity. For the heat neceſſary to raiſe the temperature of ice each degree from natural zero, is to the heat neceſſary to raiſe water each degree from the ſame point as the capacity of the one body is to that of the other. Ice cannot then as it acquires its new form, ſhew any augmentation of temperature till the differences between the heats of each degree from the loweſt point be made up. This difference amounts to the 140° found by the experiment, and cannot raiſe the water even the fraction of a degree in temperature, becauſe it is barely ſufficient for the demands of its new capacity. Had the 140° been applied to water, Dr. Thompſon's aſſertion of the riſe of temperature would have been juſt, but it can have no reference whatever to heat applied during a change of capacity.

But it was not raiſed at all: and therefore the 126° of heat are conſidered as having beyond doubt *combined with the body* and fuſed it.

Reply. Dr. Thompſon in his ſtatement of the change of capacity, attends to the ſuperadded heat only, and totally overlooks the heat before exiſting in the body.

Inadmiſſible conſequence. Dr. Irvine and all writers but Dr. Thompſon have conſidered the *whole heat* in a body at a given temperature as the *meaſure of its capacity*.

Ice in fuſion *abſorbs all the heat* preſented to it until its enlarged capacity is ſatisfied, and cannot till then have any increaſe of temperature.

The ſame arguments are repeated in page 271, and the ſame miſtake reigns through the whole. Is it not obvious, that if you would inſtantaneouſly increaſe the capacity of any body, it would immediately become colder, and its temperature ſink as much lower as its new capacity was higher. In the ſame way, if a body has its capacity ſuddenly increaſed, and at the ſame

If the capacity of a body were ſuddenly increaſed its temperature would fall; or it would continue ſtationary if due heat were added;

fame time a quantity of heat added to it to make the whole heat in proportion to its new capacity, furely that body would continue exactly at the fame thermometrical point. Now melting ice is that body; the 140° are demanded by new capacity,—would have been more if its capacity had been greater, and lefs if lefs. On the very principles therefore in difpute, ice on becoming water ought to receive a quantity of heat, and that quantity is not governed by the proportion of 9 to 10, but the whole heats are in that proportion, and the 140° is only their difference. For all calculations on the alteration of temperature to be produced by a given portion of heat on a body, from the knowledge of the relation of its capacity to another, and of the number of degrees that other is raifed by the fame quantity, continue juft only while the capacities continue in the fame proportion.

and this is the cafe with ice.

hence, &c. as before.

The caufe of fluidity afcribed to the fame action of heat which enlarges the capacities of bodies.

Surely it is poffible to form a notion that heat (140°,) may enter into water as the very caufe of fufion, fo as to alter its ftate and change its capacity; that very heat making the quantum due to the new capacity. It is the fact, that ice at 32° cannot bear the fmalleft addition of heat in that ftate, but immediately begins to be converted into water. Let a quantity much lefs than reprefented by 140°, enter a given portion of ice, fay $\frac{1}{2}$ a degree. A fmall portion of the ice becomes water; its capacity being increafed as 9 : 10, and here the procefs ftops; the entire mafs of ice and water remaining at 32°. More heat would alter the ftate and enlarge the capacity of more of the ice, without raifing its temperature; that is to fay, would fufe it. How this is done, or in other words, what it is that happens among the particles may not be eafy to explain or to imagine; but in my apprehenfion this hypothetical part of the difcuffion would be at leaft as obfcure in the doctrine of latent or combined heat, as in that which afcribes the difappearance of heat during fufions to the enlargement of capacity.

The experiments on heat are moftly too inaccurate to give the fame depreffion for the natural zero.

As to the difagreement of refults in the hands of different philofophers concerning the natural zero, it is to be obferved, that it is one thing to determine whether bodies have different capacities for heat, and another to exprefs thefe by accurate numbers. There can be no doubt of the fact that bodies have different capacities, and Dr. Irvine's theorem may enuntiate itfelf generally, by faying, that as the capacity of the folid is

to

to that of the fluid,* fo is the whole heat of the folid to that of the fluid. But it is a widely different thing to determine precifely the capacities of airs, and various other fubftances, experiments on which are fubject to great inaccuracy, and muft be carefully repeated many times before a philofopher fhould pretend to draw final, and ftill lefs elementary, conclufions. Many of the experiments by which contradictory refults have been obtained were made, (as I am told, for I have not yet had an opportunity of examining myfelf) by the calorimeter, an inftrument liable to great and deferved objections. Lavoifier, by mixing fulphuric acid and water made the natural zero 5803: my father by a fimilar procefs fomewhere I think between 8 and 900. What will Dr. Thompfon make of this? Surely he will not conclude the theorem falfe, but one expementer wrong; moft likely both inaccurate.

Dr. Thompfon has reafoned as if the capacities of ice were not changed during the experiment.

It is very ftrange indeed that Dr. Thompfon fhould have found it difficult to underftand how thefe 140° enter ice during fufion without raifing its temperature. All he fays amounts to this, that a given quantity of heat will have more effect in raifing the temperature of ice than that of water. But the ice muft continue ice, and the water water, and a change of capacity alters the whole reafoning. Before he can tell whether 140 fhould be 14 or not, he muft tell me the whole heat of ice, and let me examine whether that be to itfelf plus, 140 as 9 : 10 or not. He is exactly in the fame error with regard to fteam.

Dr. Irvine's method of afcertaining the capacities of ice and water was by afcertaining how much fine fand produced an equal change of temperature in each.

As to a mode of finding the capacities of ice and water, of which my father was the undoubted difcoverer, as well as of the general fact that all bodies change their capacity and form together, one of Irvine's modes was this: he mixed fine river fand wafhed, or fine pounded glafs of a given temperature with each, fo as to raife or reduce each an equal number of degrees. Then the capacities were as the quantities of glafs added to produce the fame effect.

Dr. Thompfon afferts that the capacities are not proved to be as the abfolute heats.

Dr. Thompfon fays, that there is no proof that the capacities of bodies are as their abfolute heats. The capacity of iron, he continues, is greater than that of water or even that of azotic gas, yet it is improbable that iron contains more heat than thefe fubftances. Now where did Dr. Thompfon find that the capacity of iron was greater than that of water and azotic gas? not in his own table furely. There iron by weight has capacity 0.1264, water 1.000, azotic gas, 0.7036, or as Dr Crawford

Inftance of iron, faid to have greater capacity and contain lefs heat than water:—but miftakenly.

Crawford ſays, .7936. Even of equal bulks that of iron is leſs than that of water, as I ſee in the ſame table. I can only ſuppoſe that Dr. Thompſon has ſtumbled on the ſpecific gravities inſtead of ſpecific heats, and there he would have been right enough.

The ſame experiment that determines ſpecific heats is alſo the teſt of the capacities. The abſolute heats are (*by inference* extended thro' the whole range from zero) taken to be in the ſame ratio.

I will ſay only a few words farther on the queſtion, whether bodies contain caloric in proportion to their ſpecific heats or not. Now firſt of all when they continue of the ſame capacities. Suppoſe the capacity of a body to be to that of water as 10 to 5, *i. e.* double. The ſame quantity of heat that raiſes water two degrees, raiſes it one; 2° more raiſe the body one more, and ſo on as far as we can go upwards, and the reverſe downwards in the ſcale. But ſuppoſe another body whoſe capacity is to that of water as 20 to 5, *i. e.* quadruple. Each 4° of the heat in water raiſe this new body one degree upwards, and the reverſe downwards, as far as we know. Now is it not probable here that the whole heats are in proportion to the capacities thus determined, ſince like thermometrical portions of heat taken out of each and applied to water affect it in that ratio? The ſpecific heats of bodies are ſaid to be different, when the ſame quantity of heat raiſes one a different number of degrees from the other, and that regularly as far as we can examine. Therefore each degree in each contains a quantity of heat proportional to its capacity. But the whole heat is made up of degrees, therefore the whole heats are proportional if the capacities remain the ſame. Dr. Thompſon grants this to be abſolute fact to the extent of our experiments.

If the capacities ſhould vary while the temperature changes, the only conſequence would be, that heat would be given out or abſorbed till the common temperature were reſtored, and then the abſolute heats would be proportioned to the new capacities.

But if the capacity be ſuppoſed to vary, firſt let it diminiſh. Then the quantity of heat given out is the difference of the whole heats of the two different ſtates of the body; and the whole heat of it in each ſtate is proportional to its capacity, and the whole heat of its higheſt capacity is equal to that of its loweſt plus the heat given out. Therefore the change of capacity has made no alteration on the whole heat of the body computed from a higher point, but will turn out the ſame as if no change had taken place.

If the capacity be ſuppoſed to increaſe, a ſimilar reaſoning would ſhew that the heat ſtill may be computed in the ſame way. Such are a few arguments on the other ſide of the queſtion

tion from Dr. Thompfon, by no means all that might be brought, nor dare I venture to hope fo ftated as to be beyond the reach of cenfure.

Many more curious points rife before the imagination on fo interefting a fubject as heat. As I hope, however, foon to be able to lay before the public fome of my father's writings, I may on that occafion have an opportunity of expreffing myfelf at greater length than I can intrude in your journal.

Intended publication of the writings of the late Dr. Irvine.

I am,

SIR,

Your obedient humble fervant,

WILLIAM IRVINE.

Bedford-Street, Covent-Garden.

V.

An Account of fome Experiments and Obfervations on the Conftituent Parts of certain Aftringent Vegetables; and on their Operation in Tanning. By HUMPHRY DAVY, Efq. *Profeffor of Chemiftry in the Royal Inftitution.*

(Concluded from Page 256. Vol. V.*)*

IV. EXPERIMENTS AND OBSERVATIONS ON THE ASTRINGENT INFUSIONS OF BARKS, AND OTHER VEGETABLE PRODUCTIONS.

THE barks that I examined were furnifhed me by my friend Samuel Purkis, Efq. of Brentford; they had been collected in the proper feafon, and preferved with care.

Infufions of barks in water by gentle heat.

In making the infufions, I employed the barks in coarfe powder; and, to expedite the folution, a heat of from 100 to 120° Fahrenheit was applied.

The ftrongeft infufions of the barks of the oak, of the Leicefter willow, and of the Spanifh chefnut, were nearly of the fame fpecific gravity, 1.05. Their taftes were alike, and ftrongly aftringent; they all reddened litmus-paper; the infufion of the Spanifh chefnut bark producing the higheft tint; and that of the Leicefter willow bark the feebleft tint.

Of oak, willow, and Spanifh chefnut,

Two hundred grains of each of the infufions were fubmitted to evaporation; and, in this procefs, the infufion of the oak

were chemically examined.

bark

Chemical examination of various barks.

bark furnished 17 grains of solid matter; that of the Leicester willow about $16\frac{1}{2}$ grains; and that of the Spanish chesnut nearly an equal quantity.

The tannin given by these solid matters was, in that from the oak bark infusion, 14 grains; in that from the willow bark infusion $14\frac{1}{2}$ grains; and in that from the Spanish chesnut bark infusion 13 grains.

The residual substances of the infusions of the Spanish chesnut bark, and of the oak bark, slightly reddened litmus-paper, and precipitated the solutions of tin of a fawn colour, and those of iron black. The residual matter of the infusion of the willow bark, did not perceptibly change the colour of litmus; but it precipitated the salts of iron of an olive colour, and rendered turbid the solution of nitrate of alumine.

The solid matters produced by the evaporation of the infusions, gave, by incineration, only a very small quantity of ashes, which could not have been more than $\frac{1}{130}$ of their original weights. These ashes chiefly consisted of calcareous earth and alkali; and the quantity was greatest from the infusion of chesnut bark.

The infusions were acted on by the acids, and the pure alkalis, in a manner very similar to the infusion of galls. With the solutions of carbonated alkalis, they gave dense fawn-coloured precipitates. They were copiously precipitated by the solutions of lime, of strontia, and of barytes; and, by lime-water in excess, the infusions of oak and of chesnut bark seemed to be deprived of the whole of the vegetable matter they held in solution.

By being boiled for some time with alumine, lime, and magnesia, they became almost colourless, and lost their power of acting upon gelatine and the salts of iron. After being heated with carbonate of lime and carbonate of magnesia, they were found deeper coloured than before; and, though they had lost their power of acting on gelatine, they still gave dense olive-coloured precipitates with the salts of iron.

In all these cases, the earths gained tints of brown, more or less intense.

When the compound of the astringent principles of the infusion of oak bark with lime, procured by means of lime-water, was acted on by sulphuric acid, a solution was obtained, which pre-

precipitated gelatine, and contained a portion of the vegetable principles, and a certain quantity of fulphate of lime; a folid fawn-coloured matter was likewife formed, which appeared to be fulphate of lime, united to a little tannin and extractive matter.*

The folutions were copioufly precipitated by folution of albumen. Copious precipitations by albumen.

The precipitates they gave with gelatine were fimilar in their appearance; their colour, at firft, was a light tinge of brown, but they became very dark by expofure to the air. Their compofition was very nearly fimilar; and, judging from the experiments on the quantity of gelatine employed in forming them, the compound of tannin and gelatine from the ftrongeft infufion of oak bark, feems to confift, in the 100 parts, of 59 parts of gelatine and 41 of tannin; that from the infufion of Leicefter willow bark, of 57 parts of gelatine and 43 of tannin; and that from the infufion of Spanifh chefnut bark, of 61 parts of gelatine and 39 of tannin.

Two pieces of calf-fkin, which weighed when dry 120 grains each, were tanned; one in the ftrongeft infufion of Leicefter willow bark, and the other in the ftrongeft infufion of oak bark. The procefs was completed, in both inftances, in lefs than a fortnight; when the weight of the leather formed by the tannin of the Leicefter willow bark was found equal to 161 grains; and that of the leather formed by the infufion of oak bark was equal to 164 grains. Experiments of tanning fkin with the infufions of barks.

When pieces of fkin were fuffered to remain in fmall quantities of the infufions of the oak bark, and of the Leicefter willow bark, till they were exhaufted of their tanning principle, it was found, that though the refidual liquors gave olive-coloured precipitates with the folutions of fulphate of iron, yet they were fcarcely rendered turbid by folutions of muriate of tin; and there is every reafon to fuppofe, that a portion of their extractive matter had been taken up with the tannin by the fkin. Spent ouze or infufion.

* M. Merat Guillot propofes a method of procuring pure tannin, (*Annales de Chimie*, Tome XLI. p. 325.) which confifts in precipitating a folution of tan by lime-water, and decompofing it by nitric or muriatic acid. The folution of the folid matter obtained in this way in alcohol, he confiders as a folution of pure tannin; but, from the experiments above-mentioned, it appears, that it muft contain, befides tannin, fome of the extractive matter of the bark; and it *may* likewife contain faline matter.

 Lat-

I attempted, in different modes, to obtain uncombined gallic acid from the folid matter produced by the evaporation of the barks, but without fuccefs. When portions of this folid matter were expofed to the degree of heat that is required for the production of gallic acid from Aleppo galls, no cryftals were formed; and the fluid that came over gave only a brown colour to the folution of falts of iron, and was found to contain much acetous acid and empyreumatic oil.

When pure water was made to act, in fucceffive portions, upon oak bark in coarfe powder, till all its foluble parts were taken up, the quantities of liquor laft obtained, though they did not act much upon folution of gelatine, or perceptibly redden litmus-paper, produced a denfe black with the folution of fulphate of iron: by evaporation, they furnifhed a brown matter, of which a part was rendered infoluble in water by the action of the atmofphere; and the part foluble in water was not in any degree taken up by fulphuric ether; fo that, if it contained gallic acid, it was in a ftate of intimate union with extractive matter.

Slow tanning appears to add lefs to the weight of the leather than quick, and more of the mucilage is taken up.

Two pieces of calf-fkin, which weighed when dry 94 grains each, were flowly tanned; one by being expofed to a weak infufion of the Leicefter willow bark, and the other by being acted upon by a weak infufion of oak bark. The procefs was completed in about three months; and it was found, that one piece of fkin had gained in weight 14 grains, and the other piece about 16½ grains. This increafe is proportionally much lefs than that which took place in the experiment on the procefs of quick tanning. The colour of the pieces of leather was deeper than that of the pieces which had been quickly tanned; and, to judge from the properties of the refidual liquors, more of the extractive matters of the barks had been combined with them.

The experiments of Mr. Biggin * have fhown, that fimilar barks, when taken from trees at different feafons, differ as to the quantities of tannin they contain: and I have obferved, that the proportions of the aftringent principles in barks, vary confiderably according as their age and fize are different; befides, thefe proportions are often influenced by accidental circumftances, fo that it is extremely difficult to afcertain their diftinct relations to each other.

* Phil. Tranf. for 1799, page 299.

In

In every aſtringent bark, the interior white bark (that is, the part next to the alburnum) contains the largeſt quantity of tannin. The proportion of extractive matter is generally greateſt in the middle or coloured part: but the epidermis ſeldom furniſhes either tannin or extractive matter.

The interior coat of bark contains moſt tannin; the middle moſt extractive; and the epidermis little of either.

The white cortical layers are comparatively moſt abundant in young trees; and hence their barks contain, in the ſame weight, a larger proportion of tannin than the barks of old trees. In barks of the ſame kind, but of different ages, which have been cut at the ſame ſeaſon, the ſimilar parts contain always very nearly the ſame quantities of aſtringent principles; and the interior layers afford about equal portions of tannin.

Young trees afford more tannin.

An ounce of the white cortical layers of old oak bark, furniſhed, by lixiviation and ſubſequent evaporation, 108 grains of ſolid matter; and, of this, 72 grains were tannin. An equal quantity of the white cortical layers of young oak produced 111 grains of ſolid matter, of which 77 were precipitated by gelatine.

Relative quantities.

An ounce of the interior part of the bark of the Spaniſh cheſnut, gave 89 grains of ſolid matter, containing 63 grains of tannin.

The ſame quantity of the ſame part of the bark of the Leiceſter willow, produced 117 grains, of which 79 were tannin.

An ounce of the coloured or external cortical layers from the oak, produced 43 grains of ſolid matter, of which 19 were tannin.

From the Spaniſh cheſnut, 41 grains, of which 14 were tannin.

And, from the Leiceſter willow, 34 grains, of which 16 were tannin.

In attempting to aſcertain the relative quantities of tannin in the different *entire* barks, I ſelected thoſe ſpecimens which appeared ſimilar with regard to the proportions of the external and internal layers, and which were about the average thickneſs of the barks commonly uſed in tanning, namely, half an inch.

Of theſe barks, the oak produced, in the quantity of an ounce, 61 grains of matter diſſolved by water, of which 29 grains were tannin.

The Spaniſh cheſnut 53 grains, of which 21 were tannin.

And the Leiceſter willow 71 grains, of which 33 were tannin.

The

The proportions of thefe quantities, in refpect to the tanning principle, are not very different from thofe eftimated in Mr. Biggin's table.*

Properties of the refidual portion of infufed barks.

The refidual fubftances obtained in the different experiments, differed confiderably in their properties; but certain portions of them were, in all inftances, rendered infoluble during the procefs of evaporation. The refiduum of the chefnut bark, as in the inftance of the ftrongeft infufion, poffeffed flightly acid properties; but more than $\frac{1}{4}$ of its weight confifted of extractive matter. All the refiduums in folution, as in the other cafes, were precipitated by muriate of tin; and, after this precipitation, the clear fluids acted much more feebly than before on the falts of iron; fo that there is great reafon for believing, that the power of aftringent infufions to precipitate the falts of iron black, or dark coloured, depends partly upon the agency of the extractive matters they contain, as well as upon that of the tanning principle and gallic acid.

Elm and willow bark.

In purfuing the experiments upon the different aftringent infufions, I examined the infufions of the bark of the elm and of the common willow. Thefe infufions were acted on by reagents, in a manner exactly fimilar to the infufions of the other barks: they were precipitated by the acids, by folutions of the alkaline earths, and of the carbonated alkalis; and they formed, with the cauftic alkalis, fluids not precipitable by gelatine.

An ounce of the bark of the elm, furnifhed 13 grains of tannin.

The fame quantity of the bark of the common willow, gave 11 grains.

The refidual matter of the bark of the elm, contained a confiderable portion of mucilage; and that of the bark of the willow, a fmall quantity of bitter principle.

Infufions of fumachs from Sicily and Malaga.

The ftrongeft infufions of the fumachs from Sicily and Malaga, agree with the infufions of barks, in moft of their properties; but they differ from all the other aftringent infufions that have been mentioned, in one refpect; they give denfe precipitates with the cauftic alkalis. Mr. Prouft has fhown, that fumach contains abundance of fulphate of lime; and it is probably to this fubftance that the peculiar effect is owing.

From an ounce of Sicilian fumach, I obtained 165 grains of matter foluble in water, and, of this matter, 78 grains were tannin.

* Philofophical Tranfactions for 1799, p. 263.

An

An ounce of Malaga fumach, produced 156 grains of foluble matter, of which 79 appeared to be tannin.

Of Myrobalans.

The infufion of Myrobalans * from the Eaft Indies, differed from the other aftringent infufions chiefly by this circumftance, that it effervefced with the carbonated alkalis; and it gave with them a denfe precipitate, that was almoft immediately rediffolved. After the tannin had been precipitated from it by gelatine, it ftrongly reddened litmus-paper, and gave a bright black with the folutions of iron. I expected to be able to procure gallic acid, by diftillation from the Myrobalans; but in this I was miftaken; they furnifhed only a pale yellow fluid, which gave merely a flight olive tinge to folution of fulphate of iron.

Skin was fpeedily tanned in the infufion of the Myrobalans; and the appearance of the leather was fimilar to the appearance of that from galls.

The ftrongeft infufions of the teas are very fimilar, in their agencies upon chemical tefts, to the infufions of catechu.

An ounce of Souchong tea, produced 48 grains of tannin.

The fame quantity of green tea, gave 41 grains.

Dr. Maton has obferved, that very little tannin is found in cinchona, or in the other barks fuppofed to be poffeffed of febrifuge properties. My experiments tend to confirm the obfervation. None of the infufions of the ftrongly bitter vegetable fubftances that I have examined, give any precipitate to gelatine. And the infufions of quaffia, of gentian, of hops, and of chamomile, are fcarcely affected by muriate of tin; fo that they likewife contain very little extractive matter.

In all fubftances poffeffed of the aftringent tafte, there is great reafon to fufpect the prefence of tannin; it even exifts in fubftances which contain fugar and vegetable acids. I have found it in abundance in the juice of floes; and my friend Mr. Poole, of Stowey, has detected it in port wine.

V. GENERAL OBSERVATIONS.

Probability that there are different fpecies of tanning matter.

Mr. Prouft has fuppofed, in his paper upon tannin and its Species,† that there exift different fpecies of the tanning principle, poffeffed of different properties, and different powers

* The Myrobalans ufed in thefe experiments are the fruit of the Terminalia Chebula. Retz. *Obs. Botan.* Fafc. V. p. 31.

† *Annales de Chimie*, Tome XLI. p. 332.

of acting upon re-agents, but all precipitable by gelatine. This opinion is sufficiently conformable to the facts generally known concerning the nature of the substances which are produced in organised matter; but it cannot be considered as proved, till the tannin in different vegetables has been examined in its pure or insulated state. In all the vegetable infusions which have been subjected to experiment, it exists in a state of union with other principles; and its properties must necessarily be modified by the peculiar circumstances of its combination.

The specific agencies of tannin in all infusions are the same.

From the experiments that have been detailed it appears, that the *specific* agencies of tannin in all the different astringent infusions are the same. In every instance, it is capable of entering into union with the acids, alkalis, and earths; and of forming insoluble compounds with gelatine, and with skin. The infusions of the barks affect the greater number of re-agents in a manner similar to the infusion of galls; and, that this last fluid is rendered green by the carbonated alkalis, evidently depends upon the large proportion of gallic acid it contains. The infusion of sumach owes its characteristic property, of being precipitated by the caustic alkalis, to the presence of sulphate of lime; and, that the solutions of catechu do not copiously precipitate the carbonated alkalis, appears to depend upon their containing tannin in a peculiar state of union with extractive matter, and uncombined with gallic acid or earthy salts.

Its affinities and habitudes.

In making some experiments upon the affinities of the tanning principle, I found that all the earths were capable of attracting it from the alkalis: and, so great is their tendency to combine with it, that, by means of them, the compound of tannin and gelatine may be decomposed without much difficulty; for, after pure magnesia had been boiled for a few hours with this substance diffused through water, it became of a red-brown colour, and the fluid obtained by filtration produced a distinct precipitate with solution of galls. The acids have less affinity for tannin than for gelatine; and, in cases where compounds of the acids and tannin are acted on by solution of gelatine, an equilibrium of affinity is established, in consequence of which, by far the greatest quantity of tannin is carried down in the insoluble combination. The different neutral salts have, comparatively, feeble powers of attraction for the tanning principle; but, that the precipitation they occasion in astringent solutions,

is

is not simply owing to the circumstance of their uniting to a portion of the water which held the vegetable substances in solution, is evident from many facts, besides those which have been already stated. The solutions of alum, and of some other salts which are less soluble in water than tannin, produce, in many astringent infusions, precipitates as copious as the more soluble saline matters; and sulphate of lime, and other earthy neutral compounds, which are, comparatively speaking, insoluble in water, speedily deprive them of their tanning principle.

From the different facts that have been stated, it is evident that tannin may exist in a state of combination in different substances, in which its presence cannot be made evident by means of solution of gelatine; and, in this case, to detect its existence, it is necessary to have recourse to the action of the diluted acids. It is not always exhibitable by gelatine.

In considering the relations of the different facts that have been detailed, to the processes of tanning and of leather-making, it will appear sufficiently evident, that when skin is tanned in astringent infusions that contain, as well as tannin, extractive matters, portions of these matters enter, with the tannin, into chemical combination with the skin. In no case is there any reason to believe that gallic acid is absorbed in this process; and M. Seguin's ingenious theory of the agency of this substance, in producing the deoxigenation of skin, seems supported by no proofs. Even in the formation of glue from skin, there is no evidence which ought to induce us to suppose that it loses a portion of oxigen; and the effect appears to be owing merely to the separation of the gelatine, from the small quantity of albumen with which it was combined in the organised form, by the solvent powers of water. Skins in tanning absorb extractive matter;

The different qualities of leather made with the same kind of skin, seem to depend very much upon the different quantities of extractive matter it contains. The leather obtained by means of infusion of galls, is generally found harder, and more liable to crack, than the leather obtained from the infusions of barks; and, in all cases, it contains a much larger proportion of tannin, and a smaller proportion of extractive matter. and the leather has its quality affected by it;

When skin is very slowly tanned in weak solutions of the barks, or of catechu, it combines with a considerable proportion of extractive matter; and, in these cases, though the increase Soft durable leather by slow tanning.

of

of weight of the ſkin is comparatively ſmall, yet it is rendered perfectly inſoluble in water; and is found ſoft, and at the ſame time ſtrong.

The ſaturated aſtringent infuſions of barks contain much leſs extractive matter, in proportion to their tannin, than the weak infuſions; and, when ſkin is quickly tanned in them, common experience ſhows that it produces leather leſs durable than the leather ſlowly formed.

The common opinion in favour of what is called feeding of the leather probably juſt.

Beſides, in the caſe of quick tanning by means of infuſions of barks, a quantity of vegetable extractive matter is loſt to the manufacturer, which might have been made to enter into the compoſition of his leather. Theſe obſervations ſhow, that there is ſome foundation for the vulgar opinion of workmen, concerning what is technically called the *feeding* of leather in the ſlow method of tanning; and, though the proceſſes of the art may in ſome caſes be protracted for an unneceſſary length of time, yet, in general, they appear to have arrived, in conſequence of repeated practical experiments, at a degree of perfection which cannot be very far extended by means of any elucidations of theory that have as yet been made known.

On the firſt view it appears ſingular that, in thoſe caſes of tanning where extractive matter forms a certain portion of the leather, the increaſe of weight is leſs than when the ſkin is combined with pure tannin; but the fact is eaſily accounted for, when we conſider that the attraction of ſkin for tannin muſt be probably weakened by its union with extractive matter; and, whether we ſuppoſe that the tannin and extractive matter enter together into combination with the matter of ſkin, or unite with ſeparate portions of it, ſtill, in either caſe, the primary attraction of tannin for ſkin muſt be, to a certain extent, diminiſhed

Vegetables are of value for tanning not merely by the matter that glue can precipitate.

In examining aſtringent vegetables in relation to their powers of tanning ſkin, it is neceſſary to take into account, not only the quantity they contain of the *ſubſtance* precipitable by gelatine, but likewiſe the quantity, and the nature, of the extractive matter; and, in caſes of compariſon, it is eſſential to employ infuſions of the ſame degree of concentration.

Catechu is the moſt powerful tanning material.

It is evident, from the experiments detailed in the IIId ſection, that of all the aſtringent ſubſtances which have been as yet examined, catechu is that which contains the largeſt proportion of tannin; and, in ſuppoſing, according to the common eſtimation, that from four to five pounds of common oak bark are

are required to produce one pound of leather, it appears, from the various synthetical experiments, that about half a pound of catechu would anſwer the ſame purpoſe *.

Its comparative value.

Alſo, allowing for the difference in the compoſition of the different kinds of leather, it appears, from the general detail of facts, that one pound of catechu, for the common uſes of the tanner, would be nearly equal in value to 2¼ pounds of galls, to 7½ pounds of the bark of the Leiceſter willow, to 11 pounds of the bark of the Spaniſh cheſnut, to 18 pounds of the bark of the elm, to 21 pounds of the bark of the common willow, and to 3 pounds of ſumach.

Lime and other additions are probably hurtful.

Various menſtruums have been propoſed for the purpoſe of expediting and improving the proceſs of tanning, and, amongſt them, lime-water and the ſolutions of pearl-aſh: but, as theſe two ſubſtances form compounds with tannin which are not decompoſable by gelatine, it follows that their effects muſt be highly pernicious; and there is very little reaſon to ſuppoſe, that any bodies will be found which, at the ſame time that they increaſe the ſolubility of tannin in water, will not likewiſe diminiſh its attraction for ſkin.

VI.

An eaſy Method of raiſing Water for the Purpoſes of Refrigeration in Diſtilleries, Steam Condenſers, &c. By SIR A. N. EDELCRANTZ. *Communicated by the Inventor.*

Introduction.

THE method exhibited in the ſketch, Fig. II. Plate IV. being capable of ſaving near thirty feet of the height to which water may be required to be pumped, for the uſes mentioned in the title, appeared too ſimple and ingenious, when the learned inventor did me the honour to mention it in converſation, for me not to require permiſſion to communicate it to my readers.

* This eſtimation agrees very well with the experiments lately made by Mr. Purkis, upon the tanning powers of Bombay catechu in the proceſſes of manufacture, and which he has permitted me to mention. Mr. Purkis found, by the reſults of different accurate experiments, that one pound of catechu was equivalent to ſeven or eight of oak bark.

The principle of the syphon applied to produce a current of water through a worm tub.

If the worm tube were open at the top as usual, it is evident that all the water employed for cooling, would require to be raised by some mechanic force as high as the surface; suppose twenty feet. But as this water is not wanted for use at that elevation, but is only required to give out its heat, and then fall down again; it is clear that this fall may be applied to raise a considerable portion of what is to follow. Various means might be devised for such an application; the simplest and most effectual, no doubt, is that to which Sir A. N. has given the preference, namely to convert the whole apparatus into a syphon.

Description of the apparatus.

Suppose the worm tub to be closed at top; the cold water conveyed into it at the bottom from the vessel A, and carried off heated at top by the pipe B into the overflowing vessel C. Let us suppose the level in A to be two feet higher than that in C, and a current will be kept up through the whole fluid as long as may be desired.

Whether the escape of gas would prevent or impede the effect.

It must occur to the experienced engineer that gas or air will escape from the water, especially when heated and defended from the pressure of the atmosphere. But this may be obviated by attending to a few necessary circumstances. First, the elevation need not be very great, and less gas will escape; secondly, the temperature may be kept down by a large current through pipes of considerable diameter; and thirdly, it is practicable by various contrivances, that an interior float shall give notice when the gas has lowered the surface of the water beneath it to a certain point, and this may either warn an attendant to pump it out, or it may discharge an apparatus to produce the same effect without the immediate exertion of labour each individual time.

Concerning these and other secondary points, I shall not, however, enlarge; having enough to regret from the necessary imperfection of this description, taken from the conversation of the inventor, instead of being given in his own words.

W. N.

Description

VII.

Description of a new Padlock of Security with Combinations. By CITIZEN REGNIER.*

THE intention of this padlock is to secure portmanteaus, cloak bags, and other packages in the most complete manner, and to serve occasionally as defences to the key-holes of the doors of apartments. Description of a padlock of combination.

The padlock is composed of four circular pieces of brass, on which are engraven the twenty-four letters of the alphabet. The four pieces are moveable on their axes by turning them with the finger in order to produce the combination by which it is opened.

The combination of the manufacturer is the word ROME: when this word is brought into a correct line with the two marks on the edges of the two steel plates FF, which form the external part of the padlock, those two plates can be separated a little from each other, and the clasp of the lock can be opened by the hinge.

The same process is used to fasten it, with this difference, that the two external plates are pressed together so as to confine the bow or clasp of the lock in its cell at G; after which, the combination is to be shifted so that the characters shall no longer form the same word in the before-mentioned line.

The Method by which the Possessor may dispose the Padlock to act by a new Combination, which cannot be known to any other Person:

1. A screw is taken out, which passes through the centre of the plates FF.

2. The combination which it is intended to set aside, namely, that which opens the lock at present must be duly arranged.

3. The marked circular pieces or rings must be taken off from four plates of brass which constitute the central part, and together form the centre-piece of the mechanism.

4. Lastly, The rings must be replaced on the centre pieces, each according to the letter the possessor may have chosen.

For example: If you would adopt the word LOCK for the combination, the letter L of the first ring must be placed over

* Translated from a paper circulated by the constructor.

Description of a padlock of Combination.

or upon a small steel tooth, which is attached to the first interior cylinder; the letter O of the second ring on the tooth of the second cylinder; the letter C of the third ring on the tooth of the third cylinder; and lastly, the letter K of the fourth tooth on the fourth cylinder.

By this means the word *lock* is set up and becomes the combination of the lock, and the word *Rome* no longer produces the disposition of parts required for the disengagement.

After this operation the screw must be replaced in the centre of the plate; this screw does not contribute to the strength of the mechanism; but is used merely to allow the exact space necessary for opening the padlock, and to prevent the separation of the rings from the central parts in the common use of the lock.

Method of using this Lock as a Defence to the Key-hole of a Door.

A ring staple A, having a wood screw, is fixed to the door above the key-hole or escutcheon of the lock.

A second ring C is fixed perpendicularly beneath the other.

A cylindrical tube of iron D, in the form of a bolt, is placed vertically in the ring of these screw staples. At the lower end of the tube is an aperture, through which the padlock is inserted, so that the tube or bolt cannot be raised or taken out.

By this contrivance the key-hole of the lock is completely defended, and the introduction of a pick-lock or false key is rendered morally impossible. For the mechanism presents 331,776 combinations,* forming 331,775 different obstacles, to prevent the removal of this defence by any person unacquainted with the secret of the proprietor.

If it be apprehended that the word of the combination may be forgotten, it will be easy to write and disguise it in many different ways, without any risk of discovery: for example,

The letter L, or eleventh letter of the alphabet will be written	£. 11.
The letter O, or fourteenth letter - - - -	14.
The letter C, or third letter - - - - -	3.
The letter K, or tenth letter - - - -	10.
Total (expressed)	£. 38.

* Number equal to the 4th power of 24.

This

This little calculation will appear to any other person to be a common account, but it is to the proprietor a memorandum by which he will perfectly recollect that the first letter of his combination is the eleventh of the alphabet, that the second is the fourteenth, and so of the rest. Description of a padlock of combination.

REMARKS.

This padlock appears at first sight similar to that contrived by Cardan; but we know that his was not capable of having its combinations changed; whence it results that the manufacturer, the retailer, and every other person who may have seen it opened, can themselves open it with the same facility as the proprietor himself. The notches which produce the opening may be also discovered by the feel; our lock has false notches cut in the centre-piece of the mechanism which prevent the discovery of the real one.

Lastly, The clasp of this padlock is made of hardened and tempered steel, to prevent its being easily cut by an ordinary file. It is annealed so far only as to prevent its breaking.

ANNOTATION. *W. N.*

The remaining part of this paper contains the address of the inventor and vendor, C. Regnier, *ci devant Jacobins, Rue Dominique, F. St. Germain à Paris,* and also a certificate of honourable mention, &c. from the *Athenée des Arts.*

The lock of Cardan consists of the four visible circular parts carrying the alphabet. These as well as the central parts of the present lock are perforated half way through their centers by an hole, and quite through by a smaller hole, in the side of which last there is a notch extended to the circumference of the larger hole. All the four pieces are placed upon a central pin, which has side projections answering respectively to the notches, but occupying the space of the larger hole while the lock is closed. From this construction it is evident that the lock cannot be opened unless every one of the notches be placed opposite its projection; that this position or placing is settled by the maker, and not variable; and that the lock is liable to be opened, though not easily, by the tentative process described at p. 204 of our last volume.

Cit. Regnier has perfected the lock of Cardan by making the system of the alphabet moveable with regard to the internal

Description of a padlock of combinations.

nal notch at the pleasure of the possessor, and also by making groves or small notches on the face of each central piece, which answer the purpose of the teeth recommended at the page last quoted, by preventing the rings from being turned round while any pull is made against the closure.

I find some obscurity in his description of the manner of connecting the central piece and the external engraved part. From the operation, I apprehend, 1. that each ring has a number of notches at its inner surface, that answer to the letters on its outer face; 2. that each central round piece fits the cavity of its ring, and is prevented from turning by a tooth which it lodges in one of the notches; 3. that when all the four teeth are ranged in a line between F and F, the lock will open; and therefore, 4. when any particular letter is placed over the tooth, that letter becomes the effective letter for its own ring.

VIII.

Observations on the Quantity of horizontal Refraction; with a Method of measuring the Dip at Sea. By WILLIAM HYDE WOLLASTON, *M. D. F. R. S.**

Former paper of the author, and of Monge, upon horizontal refraction.

IN a Paper which I some time since presented to this Society, (printed in the Phil. Transf. for 1800,) I endeavoured to ascertain the causes, and to explain the various cases, of horizontal refraction, which I had either observed myself, or had seen described by others.

At the time of writing that essay, I had not met with the *Mémoires sur l'Egypte,* published but a short time before; and I was not aware that an account had been given by M. Monge, of the phenomenon known to the French by the name of *mirage*, which their army had daily opportunities of seeing, in their march through the deserts of Egypt.

In the perusal of this memoir, I could not fail to derive instruction from the information it contained; but, as the facts related by him accord entirely with the theory that I had advanced, I was by no means induced to adopt the explanation that he has proposed, in preference to my own.

* From the Philosophical Transactions for 1803.

The definite reflecting furface which he fuppofes to take place between two ftrata of air of different denfity, is by no means confiftent with that continued afcent of rarefied air which he himfelf admits; and the explanation founded on this hypothefis will not apply to other cafes, which may all be fatisfactorily accounted for, upon the fuppofition of a gradual change of denfity, and fucceffive curvature of the rays of light by refraction. Objection to the theory of Monge.

I have fince learned that the fame fubject has alfo been ably treated by Mr. Woltman, in Gilbert's *Annalen der Phyfik*; but I have to regret that his differtation, as well as that of Gruber, in the fame Annals, were written in a language that was unknown to me, and that I could not avail myfelf of the affiftance that I might otherwife have received from their refearches. The fubject well treated by Woltman.

When I formerly engaged in this inquiry, being impreffed with the advantage to be derived from it to nautical aftronomy, on account of the variations in the dip of the apparent horizon, from which all obfervations of altitude at fea muft neceffarily be taken, I fuggefted the expediency of a feries of obfervations, to be made by a perfon attentive to thofe changes of temperature or moifture of the atmofphere, on which he might find the depreffion of his horizon principally to depend. I had at that time no expectation that I could myfelf purfue this fubject farther to any ufeful purpofe, having little profpect of refiding for a fufficient length of time in view of the fea, and feeing no other method by which the fame end might be accomplifhed. I have, however, fince that time, found means to fatisfy myfelf, by obfervations over the furface of the Thames, that although the quantity of refraction varies in general with any change of the thermometer or hygrometer, yet the law of thefe variations is not altogether fo fimple as I had hoped it might be found. Obfervations over the furface of the Thames.

I fhall, on the prefent occafion, firft relate the facts on which this opinion is founded, and which are in themfelves fufficiently remarkable, on account of the unexpected quantity of refraction obfervable over a fhort extent of water; I fhall, in the next place, fhew that the exact determination of the concurrent changes of the atmofphere are of lefs value, and their irregularities of lefs confequence, than I had conceived, as there is a very eafy method whereby the quantity of dip at fea may be at any time correctly meafured; and therefore the end which I fought by indirect means, may be at once directly attained. Narrative.

The

Apparent diſtortion of the parts of veſſels ſeen over the ſurface of the Thames.

The firſt inſtance that occurred to me, of obſervable refraction over the ſurface of the Thames, was wholly accidental. I was ſitting in a boat near Chelſea, in ſuch a poſition that my eye was elevated about half a yard from the ſurface of the water, and had a view over its ſurface, that probably ſomewhat exceeded a mile in length, when I remarked that the oars of ſeveral barges at a diſtance, that were then coming up with the tide, appeared bent in various degrees, according to their diſtance from me. The moſt diſtant appeared nearly in the form repreſented, Plate I. Fig. 1. *d d* being my viſible horizon by apparent curvature of the water; *a b* the oar itſelf in its inclined poſition; and *b c* an inverted image of the portion *b e*. By a little attention to other boats, and to buildings on ſhore, I could diſcern that the appearance of all diſtant objects ſeen near the ſurface of the water was affected in a ſimilar manner, but that ſcarcely any of them afforded images ſo perfectly diſtinct as the oblique line of an oar dipped in the water.

Theſe effects are different from what might be cauſed by reflection.

A perſon preſent at the time (as well as ſome others to whom I have ſince related the circumſtance) was inclined to attribute the appearance to reflection from the ſurface of the water; but, by a moderate ſhare of attention, a very evident difference may be diſcovered between the inverſion occaſioned by reflection, and that which is cauſed by atmoſpherical refraction. In caſes of reflection, the angles between the object and image are ſharp, the line of contact between them ſtraight and well defined, but the lower part of the image indefinite and confuſed, by means of any ſlight undulation of the water. But, when the images are cauſed by refraction, the confines of the object and its inverted image are rounded and indiſtinct, and the lower edge of the image is terminated by a ſtraight line at the ſurface of the water. In addition to theſe marks of difference, there is another circumſtance which, if attended to, muſt at once remove all doubt; for, by bringing the line of ſight near to the ſurface of the water, boats and other ſmall objects are found to be completely hidden by an apparent horizon, which, in ſo ſhort a diſtance, cannot be owing to any real curvature of the water, and can ariſe ſolely from the bending of the rays by refraction.

They appear referrible to an higher temperature in the

When I reflected upon the cauſes which were probably inſtrumental in the production of theſe phenomena, they appeared referrible

referrible to difference of temperature alone. After a succession of weather so hot that the thermometer, during one month preceding, had been 12 times above 80°, and on an average of the month at 68°, the evening of that day (August 22, 1800) was unusually cold, the thermometer being 55°. The water might be supposed to retain the temperature it had acquired during a few weeks preceding, and, by warming the stratum of air immediately contiguous to it, might cause a diminution of its refractive density, sufficient to effect this inverted curvature of the rays of light, in the manner formerly explained. As I was at that time unprovided with instruments of any kind, I had it not in my power to estimate the quantity of refraction, or temperatures; and can only say that, to my hand, the water felt in an uncommon degree warmer than the air.

water, which being communicated to the lowest stratum of the air, alters its refractive power.

Being thus furnished with an unexpected field for observation, I from that time took such opportunities as similar changes of the weather afforded me, of examining and measuring the quantities of refraction that might be discovered by the same means over another part of the river, that I found most suited to my convenience.

Particular account of the observations.

The situation from which the greater part of my observations were made, was at the S. E. corner of Somerset house. The view from this spot extends under Blackfriars bridge, towards London bridge, upwards of a mile in length, and in the opposite direction through Westminster bridge, which is three quarters of a mile distant.

Such distances are however by no means necessary; and indeed the air over the river, in cold weather, is generally, or at least very frequently, not sufficiently clear for seeing distinctly to so great distances. For, since the winds which are most likely to effect a sufficient change of temperature, on account of their coldness, are usually from the E. or N. E. the principal smoke of the town is then brought in that direction, and hovers, like a dense fog over the course of the river. This circumstance deprived me of many opportunities which the changes of the thermometer indicated to be favourable for my purpose, and obliged me often to make use of shorter distances than I should otherwise have chosen, by bringing the line of sight as near as I could to the surface of the water.

For this purpose, I had a plane reflector fitted to the object-end of a small pocket telescope, at an angle of 45°, so that,

 when

when the telefcope was held vertically, it gave a horizontal view at any level that was found moft eligible. When the water has been calm, I have obferved that the greateft refraction was vifible within an inch or two of its furface, and I have then feen a refraction of fix or feven minutes in the fpace of 300 or 400 yards: at other times, I have found it greateft at the height of a foot or two; but, in this cafe, a far more extenfive view becomes neceffary.

The firft meafures that I took were on the 23d of September, 1800. The water was $2\frac{1}{2}°$ warmer than the air, and I found a refraction of about 4′.

Oct. 17. The difference of temperature was 3°, and the refraction 3′.

Oct. 22. The water was $11\frac{1}{2}°$ warmer than the air, yet the quantity of refraction did not exceed 3′.

The fmallnefs of the quantity of refraction upon this occafion, I attributed to the drynefs of the atmofphere, conjecturing that a rapid evaporation might in great meafure counteract that warmth which the water would otherwife have communicated to the air.

From that time, therefore, I have noted not only the heights of the thermometer in the water and in the air, but have added alfo the degrees of cold produced by keeping the bulb of it moiftened for a fufficient time to render it ftationary. In confirmation of my conjecture refpecting the drynefs of Oct. 22, I have alfo, in the following Table, which comprifes the whole of my obfervations, inferted a column from the Regifter kept at the apartments of the Royal Society, containing the heights of the hygrometer, on thofe mornings when my obfervations were made.

TABLE.

TABLE.

At 8, A. M.	Air.	Water.	Diffe-rence.	Refrac-tion.	Cold by evaporation.	Hygro-meter.
1800. Sept. 23	57	60½°	3½°	4′	— —	72°
Oct. 17	46½	49½	3	3	— —	72
22	38	49½	11½	3	— —	67
Nov. 1	41	45½	4½	8	½°	76
4	43¾	46¾	3	3 —	1¼	72
5	37	45	8	8 +	1	69
12	44½	48½	4	1 +	3½	73
13	40	44½	4½	5	½	76
1801. June 13	50	63	13	9 +	5	65
22	55	61	6	6 +	6	65
23	55	62	7	6	4½	65
24	55	61	6	5	3	67
Sept. 8	60	64	4	7	2	78
9	64	64¼	¼	5	3	74
10	58	64	6	7	2	70
12 o'clock, 10	63	64	1	2		

Table of observations.

From a review of the preceding Table it will be found, upon the whole, that when the water is warmer than the air, some increase of depression of the horizon may be expected; but that its quantity will be greatly influenced, and in general diminished, by dryness of the atmosphere.

The dip of the visible horizon will be increased by the water being warmer than the air; and diminished (more considerably) by dryness in the air.

It appears, however, that no observable regularity is deducible from the measures above given; but that the quantity, on some occasions, is far different from what the states of the thermometer and hygrometer would indicate. On the 9th of September, for instance, the difference of temperature is only ¼°, and the evaporation, to counteract this slight excess of warmth, produced as much as 3° of cold; nevertheless, the refraction visible was full 5′. In this observation I think that I could not be mistaken, as the water was at the time perfectly calm, the air uncommonly clear, and I had leisure to pay particular attention to so unforeseen an occurrence.

The measures indicate little of any practical law.

This one instance appears conformable to the opinion entertained by Mr. Huddart, and by M. Monge, that, under some circumstances, the solution of water in the atmosphere causes a decrease in its refractive power; but, on no other occasion have I been induced to draw a similar inference.

Solution of moisture not of much probable consequence.

The

The object that I have at all times chosen, as shewing best the quantity of refraction, has been either an oar dipped in the water at the greatest discernible distance, or some other line equally inclined; and the angle measured has been, from the point where the inverted image is terminated by the water, to that part of the oar itself which appears to be directly above it. (The apparent magnitude of *ec*, Fig. 1. Pl. 3.)

The eight first angles were taken with a mother-of-pearl micrometer in the principal focus of my telescope, and are not so much to be depended upon for accuracy as the succeeding eight. These last were measured with a divided eye-glass micrometer, and consequently are not liable to any error from unsteadiness of the instrument or object.

It is not likely that so great a variation of the dip happens out at sea.

From the foregoing observations we learn, that the quantity of refraction over the surface of water may be very considerable, where the land is near enough to influence the temperature of the air. At sea, however, so great differences of temperature cannot be expected; and the increase of dip caused by this variation of horizontal refraction, it is to be presumed, is not so great as in the confined course of a river; but, if we consider that it may also be subject to an equal diminution from an opposite cause, and that the horizon may even become apparently elevated, there can be no question that the error in nautical observations, arising from a supposition that it is invariably according to the height of the observer, stands in need of correction.

Mr. Huddart's remedy.

The remedy employed by Mr. Huddart,* of taking two angles of the sun from opposite points of the horizon at the same time, and considering the excess of their sum above 180° as double the dip, must without doubt be effectual; but, from causes which he assigns, it is practicable only within certain limits of zenith distance; for, where the zenith distance is small, and the changes of azimuth rapid, there is required considerable dexterity and steadiness of a single observer who attempts to turn in due time, from one observation to another; and, when it exceeds 30°, the greater angle cannot be measured with a sextant, and consequently his method is, with that instrument, of use only in low latitudes.

* Phil. Trans. for 1797, p. 40.

On

On account of the difficulty attending fome of the adjuftments for the back obfervation, he rejects that method for taking angles in general, with much reafon; but he has thereby overlooked a means of determining the dip, which I am inclined to think might be employed with advantage in all latitudes, without any occafion to hurry the moft inexperienced or cautious obferver.

Objections to the back obfervation of angles,

By the back obfervation, the whole vertical angle between any two oppofite points of the horizon may be meafured at once, either before or after taking an altitude. Half the excefs of this angle above 180°, fhould of courfe be the dip required.

but the method recommended for meafuring the arc between the oppofite horizons.

But, if it be doubtful whether the inftrument is duly adjufted, a fecond obfervation becomes neceffary. The inftrument muft be reverfed, and, if the apparent deficiency of the oppofite angle from 180° be not equal to the excefs before obtained, the index error may then be corrected accordingly; and, fince the want of adjnftment, either of the glaffes at right angles to the plane of the inftrument, or of the line of fight parallel to it, will affect both the larger and fmaller angle very nearly in an equal degree, the ¼ part of their difference will be extremely near the truth, and the errors arifing from want of thofe adjuftments may with fafety be neglected.

Error of the glaffes found by reverfing the whole inftrument,

This method of correcting the index error for the back obfervation at fea, was many years fince recommended by Mr. Ludlam;* yet I do not find that it has been noticed by fubfequent writers on that fubject, or fuggefted by any one for determining the dip; but I can difcover no reafon for which it could be rejected as fallacious, and I fhould hope that in practice it would be found convenient, fince in theory it appears to be effectual.

as firft fuggefted by Ludlam.

The moft obvious objection to this, as well as to Mr. Huddart's method, is the poffibility that the refraction may be in fome meafure different in oppofite points of the horizon at the fame time. When land is at no great diftance, fuch an inequality may be found to occur; but, upon the furface of the ocean in general, any partial variations of temperature can rarely be fuppofed to exift; and it is probable, that under any circumftances, the difference will not bear any confiderable

Whether refraction may be different on different bearings at the fame time.

* Directions for the ufe of Hadley's quadrant, 1771, § 82, p. 56.

proportion

proportion to the whole refraction; nor can it be thought a sufficient reason for rejecting one correction proposed, that there may yet remain other small errors, to which all methods are equally liable, but which it is not the object of the present dissertation to rectify.

IX.

An Account of Two Halos, with Parhelia. By Sir H. C. ENGLEFIELD, *Bart.* F. R. S*.

Uncommon halos round the sun.

ON the 20th of November, 1802, at two o'clock in the afternoon, going out of doors, at Richmond in Surry, I perceived the sun, accompanied by uncommon halos and parhelia. The weather was showery, and the sky had that peculiar turbid appearance, which is the certain forerunner of heavy and long continued rain. The sun shone with a faint and watery light, was very ill defined, and appeared rather elongated in a vertical direction. A very dense cloud occupied all that quarter of the horizon, and rose up pretty near to the sun. Very heavy clouds covered the eastern part of the heavens, extending quite to the north, and were proceeding gradually towards the south west. The wind was about east.

The altitude of the sun was 14°. The circle nearest the sun was distant from him nearly 24°, and was about a degree in breadth. It was of a pale yellowish light, but had no tendency to prismatic colours.

On the left hand, it extended below a line drawn through the sun parallel to the horizon. To the right, it terminated in dense clouds considerably above that line.

The exterior circle was 48° from the sun, and it might be 1½° in breadth, as it was evidently broader than the inner circle. It terminated on either hand at nearly the same height as the interior one. It was tinged throughout with the prismatic colours, though only red, green, and blue, were distinctly visible. The red was nearest the sun. The blue very faint. The brightness of this circle was about that of the secondary rainbow, to a bright common bow.

* Royal Institution, II. 1.

In

In a line parallel to the horizon, paffing through the fun, there was, in the left hand branch of the inner circle; a very faint parhelion; but in the upper point of the fame circle, there was a very bright and remarkable one. Its light was fo vivid, that it could fcarcely be fteadily viewed; and, indeed, it was rather brighter than the real fun. It was of a whiter light than the reft of the circle in which it was, and had a pearly appearance, as partaking a little of prifmatic tints. It was large, perhaps in its brighteft part near two degrees broad, very ill defined every where, but moft diffufed in the part furtheft from the fun. From each fide of the bright light proceeded a bright ray, which had a double curvature very diftinct, being firft convex towards the fun, and then concave. The lower edge of thefe rays (or that neareft the fun), was tolerably well defined, the upper edge melted away into the fky, with a fort of ftreakinefs. They grew both narrower and fainter towards their termination, and they reached pretty near to the other circle.

Uncommon halos round the fun.

The whole form of this parhelion and its rays, bore fo ftriking a fimilitude to the body and extended wings of a long winged bird, fuch as an eagle, hovering directly over the fun, that fuperftition would really have had little to add to the image.

There was no trace of any other circle or arch in the heavens, nor of any anthelion.

It is probable, that it had been ftill more beautiful before I faw it, as during the time I obferved it, its brightnefs was continually diminifhing; fome traces, however, were vifible for nearly half an hour.

The meafures which I have given muft be confidered as very rough. I had no inftrument at hand, but a fix inch pocket fector. I held the joint of this as clofe to my eye as I could, and opened it, till the points of the legs coincided with the fun and with the circles that I wifhed to meafure. I am, however, inclined to think, that the meafures I have given are true within a degree.

The accompanying fketch, (Plate III.) which is drawn on a fcale of 20° to an inch, from a rough draught which I made at the moment, will give a more diftinct idea of the whole appearance than can be conveyed by words.

A Theory

A Theory of Halos and Parhelia. By THOMAS YOUNG, M. D. F. R. S.

Theory of halos and parhelia.

The explanation of the primary and fecondary rainbow begun by De Dominis, and completed by Defcartes and Newton, derives an entire and fatisfactory confirmation, from the perfect coincidence of the obferved angular magnitudes, with the refult of calculations of the effect of fpherical drops. We know that drops of water, either accurately, or very nearly fpherical, exift in great abundance in every cloud, and in every fhower of rain; and whatever their dimenfions may be, they muft neceffarily confpire in the fame general effect, of producing the fame rainbow, whenever a fpectator is placed in a proper fituation for obferving it; confequently fuch rainbows are of very frequent occurrence.

Variable halos produced by equal drops.

I have attempted to fhow, that for producing the phenomena of variable halos, often obfervable in hot climates, it is only neceffary that a confiderable part of the fpherules of a cloud or mift, be either accurately, or very nearly, of equal magnitude, a condition, of which the poffibility is eafily admitted from analogy, and the probability is favoured by the apparent uniformity of the different parts of fuch mifts as we can examine.

The conftant halo of 23° not explained.

But no fatisfactory reafon has hitherto been affigned for the production of the halo, which in thefe climates is the moft common of all; that is, the conftant halo of 23° or 24°. The hypothefis by which Huygens attempted to explain the production of halos and parhelia, are both arbitrary and improbable. He imagined the exiftence of particles of hail, fome globular, others cylindrical, with an opaque part in the middle of each, bearing a certain ratio to the whole; and he fuppofed the pofition of the cylinders to be fometimes vertical, and fometimes inclined to the horizon in a given angle.

The hypothefis of Huygens improbable.

It has already been objected, that no fuch particles have ever been obferved to accompany halos; and it is, befides, highly improbable, that fuch an opaque part fhould bear the fame proportion in all the hailftones, and that the cylinders fhould have terminations fo peculiar as is fuppofed; and the moft incredible circumftance of all is, that all thefe proportions fhould be conftantly fuch, as always to produce a halo at the diftance of 23° or 24° from the fun or moon.

It

It appears, that a much fimpler and more natural explanation of thefe phenomena may be deduced from the regular cryftallization of fnow in the atmofphere.

It is well known, that the cryftals of ice and fnow, tend always to form angles of 60°; now a prifm of water or ice, of 60°, produces a deviation of 23° 37′, for rays forming equal angles with its furfaces, and the angle of deviation varies at firft very flowly, as the inclination changes, the variation amounting to lefs than 3°, while the inclination changes 30°.

The equilateral prifmatic cryftals of ice produce a deviation of 23° 37′ which varies at firft very flowly.

Now if fuch prifms were placed at all poffible angles of inclination, differing equally from each other, one half of them would be fo fituated, as to be incapable of tranfmitting any light regularly by two fucceffive refractions directed the fame way; and of the remaining two fourths, the one would refract all the light within thefe three degrees, and the other would difperfe the light in a fpace of between 20° and 30° beyond them.

The cafual arrangement of thefe will give the halo.

In the fame manner, we may imagine an immenfe number of prifmatic particles of fnow to be difpofed in all poffible directions, and a confiderable proportion of them to be fo fituated, that the plane of their tranfverfe fection may pafs within certain limits of the fun and the fpectator. Then half of thefe only will appear illuminated, and the greater part of the light will be tranfmitted by fuch as are fituated at an angular diftance of 23° 37′, or within 3° of it: the limit being ftrongly marked internally, but the light being externally more gradually loft. And this is precifely the appearance of the moft common halo. When there is a fufficient quantity of the prifmatic particles, a confiderable part of the light muft fall, after one refraction, on a fecond particle; fo that the effect will be doubled: and, in this cafe, the angle of refraction will become fufficient to prefent a faint appearance of colour, the red being internal, as the leaft refrangible light, and the external part having a tinge of blue.

Refraction through two prifms gives the greater halo of 47°.

Thefe concentric halos of $23\frac{1}{2}$° and 47°, are therefore fufficiently explicable, by particles of fnow, fituated promifcuoufly in all poffible directions. If the prifms be fo fhort as to form triangular plates, thefe plates, in falling through the air, will tend to affume a vertical direction, and a much greater number of them will be in this fituation than in any other. The reflection from their flat furfaces will confequently produce a horizontal circle of equal height with the fun; and their refraction will

Very fhort prifms or plates will fall edgewife; and the reflection of their ends will give the horizontal circle, and their reflection a parhelion with wings.

will exhibit a bright parhelion immediately over the fun, with an appearance of wings, or horns, diverging upwards from the parhelion.

Exp. with the prifm.

For all fuch particles as are directed nearly towards the fpectator, will confpire in tranfmitting the light much more copioufly than it can arrive from any other part of the circle; but fuch as are turned more obliquely, will produce a greater deviation in the light, and at the fame time a deflection from the original vertical plane. This may be eafi' underftood, by looking at a long line through a prifm, held parallel to it: the line appears, inftead of a right line, to become a curve, the deviation being greater in thofe :ays that pafs obliquely with refpect to the axis of the prifm; which are alfo deflected from the plane in which they were paffing.

The line viewed through the prifm has no point of contrary flexure, but if its ordinates were referred to a centre, as in the cafe of the halos, it woulc' affume a form fimilar to that which Sir Henry Englefielo nas defcribed.

Tho' fnow flakes are complicated, their elements may be fimple in the upper regions, &c.

The form of the flakes of fnow as they ufually fall, is indeed more complicated than we have been fuppofing, but their elements in the upper regions of the air are probably more fimple. The coincidence in the magnitude of the obferved and calculated angles is fo ftriking, as to be nearly decifive with refpect to halos, and it is not difficult to imagine that many circumftances may exift, which may caufe the axis of the greater number of the prifms to affume a pofition nearly horizontal, which is all that is required for the explanation of the parhelia with their curved appendages. Perhaps alfo, the effect may fometimes be facilitated by the partia meiting of the fnow into conoidal drops: for it may be fhown, by the light of a candle tranfmitted through a wine glafs full of water, that fuch a form is accommodated to the production of an inverted arch of light, like that which is frequently obferved to accompany a parhelion.

A Defcription

X.

A Description of Dr. Young's *Apparatus for illustrating the Doctrine of Preponderance* *. Plate I. Fig. II.

ALTHOUGH there can be no doubt of the truth of the mathematical conclusions, which have been deduced from the well known laws of motion, respecting the most advantageous employment of force in machines, yet they have, in general, been too little consider d in practical works, and scarcely ever enforced by experimental illustration. The apparatus contrived for this purpose, has been mentioned in the account of the lectures on mechanics; its advantage is derived from the simplicity of its operation, and the facility of observing at once the several motions, which begin at the same time, and may easily be compared, as long as they continue. The ratio of the portions of the middle pulley, which is that of 5 to 2, is near enough to the maximum ($\sqrt{2} \times 1$) : 1; and the other ratios 3 : 2 and 4 : 1 are taken sufficiently different from this to show that the velocity of each is inferior to that of the middle pulley. The pulleys are all perforated in the axis, and move freely on a strong polished wire, supported by two short arms, projecting a little from two upright pieces about three feet in length, in order that the descending weights may proceed without interruption beyond the edge of the table.

Apparatus of pullies for shewing the most advantageous employment of force.

An Account of an Experiment on the Velocity of Water flowing through a Vertical Pipe. By the same Author.

IT has been asserted by some writers on hydraulics, and Venturi describes a particular experiment in support of the assertion, that the discharge of water running out of the bottom of a cistern, through a descending pipe, is nearly the same as if the cistern were continued through the whole height, from the surface of the water to the orifice of the pipe, and the water were then discharged from the bottom of the cistern by a short pipe in any direction. The apparent difficulty of finding a cause adequate to the effect, on the one hand, and the

Whether the discharge of water through a vertical pipe from the bottom of a cistern be the same as from an hole in a cistern of the total depth.

* In the lectures of the Royal Institution from whose Journal No. 11, the present and next articles are taken.

authority

authority of Venturi on the other, made it desirable that the experiment should be repeated; and an apparatus Fig. III. Plate I. was constructed, in the house of the Royal Institution, for performing it in a simple and satisfactory manner. The cistern employed was a cube of nine inches: close to the bottom a cylindrical tube was inserted, in a horizontal direction, nine inches in length, and half an inch in diameter; another tube, of exactly the same dimensions, was provided with a flat funnel at its upper end, and its lower end was fitted to slide in a collar placed in one of the upper angels of the cistern, so that it was supported in a vertical position. Water was poured into the funnel, as fast as it could be transmitted through the tube, and, as the surface of the fluid rose in the cistern, the vertical tube was drawn up, so that its lower orifice was barely immersed in the water. It was expected, that if the velocity of the water in the vertical tube were equal to the velocity corresponding to half its length, the water in the cistern would stand at the height of four inches and a half, or one half of that length, and that the pressure of this head of water would generate, in the water flowing through the horizontal tube, nearly the same velocity as the column of water would acquire in its descent through the vertical tube: the friction and resistance being in both cases the same.

Experiment shewed the contrary.

But the result was far different, and it fully confirmed the truth of the received theory: for the water rose in the cistern to the height of eight inches, which was very nearly the length of the tube. It is true that the water had already some velocity when it entered the funnel; but most of this must have been lost by reflection from its sides and bottom; and the quantity of air bubbles, that were unavoidably carried down with the water, must have fully compensated the little that remained.

The entire adhering column in the tube renders the atmosphere active upon the surface of the fluid, and produces the same effect as an head of that height.

It appears therefore, that we are to consider this effect in a light somewhat different from that in which it was placed in the lectures on hydraulics. The water acquires all its velocity, in consequence of the pressure of the atmosphere acting jointly with its cohesion, in a very small space at the entrance of the tube: consequently, during the whole time of its descent it acquires no new motion, and the whole force of its gravitation must therefore be at liberty to act in any other way; hence the whole column produces the same degree of pressure as if it were at rest, and causes the atmosphere to press on the water above

above it in proportion to its whole height, in the same manner as if the pressure were derived in any other way from an equal column of water; and the case is reduced to a perfect analogy with the pressure of a head of water of this height, since the air acts upon the particles entering the tube in the same manner as the water does in more common cases. Had the result of the experiment been different, it would have been an exception to the general principle of the preservation of living force, or the equality of the potential ascent to the actual descent; for, the water moving with the velocity due to half the height only, would have been capable of ascending but to half the height.

XI.

Account of a simple Eudiometric Apparatus constructed and used by Dr. T. C. Hope, *F. R. S. Edin.* &c. &c.

SINCE the discovery of the uncertainty with which the application of nitrous gas to atmospheric air, and other mixtures containing oxigen is attended, it has been found desirable to present solid or liquid substances for the absorption of that principle. This on first consideration may seem at least as easy to be done as to mix two gases; but it is by no means so, because the liquids in particular possess a degree of chemical activity which readers it inconvenient to immerse the hands in them, or to expose their surface to the open air. Dr. Hope, whom I had lately the pleasure of seeing in town, mentioned an apparatus he uses in his lectures and experiments, which is at once simple and effectual, and I am happy in his permission to describe it in this place.

The uncertainty of eudiometrical experiments with nitrous gas renders the use of other substances necessary.

A Fig. 3. Plate IV. represents a bottle which may be 1½ inch in diameter, and 2½ inches in length, having a neck and stopper at D, and another neck as usual at C, into which last the neck of the bottle or body B, is fitted by grinding. This last was made of the same diameter as the bottle, but seven or eight inches long. B contains the gas, and A the liquid; for example, solution of hydrosulphuret. When B is thus connected with A, the compound vessel may be inverted and agitated;

Apparatus by which fluids can be applied to air without inconvenience.

tated; and the liquid will flow into B, where it will absorb the oxigen, and form a partial vacuum. If this circumstance be found, or apprehended, to prevent the complete or rapid absorption, the vessel A may be plunged beneath the surface of common water, and the stopper D slowly opened. The pressure of the atmosphere will then force in a quantity of water, which will dilute the hydrosulphuret, but not sufficiently to prevent the completion of the process. The vessel B must be graduated to show the dimensions of the residue, or otherwise this residual gas may be transferred into a vessel expressly graduated for measuring gases.

The apparatus simply and highly convenient.

By this simple and elegant apparatus we see that the liquid is economized, and the facility, neatness, and precision of experiment insured. The size here mentioned is very well adapted to the purposes of public demonstration; but it is almost needless to remark, that it may be made considerably smaller without depriving it of its utility and excellence.

P. S. While reading this proof, I have received a line from Dr. Hope, by which I am very sorry to find that the sketch I have given is not accurate; but as it is now late in the month, and the figure is engraved, I shall be careful to give another engraving with the observations he may favour me with. W. N.

SCIENTIFIC NEWS.

*Combustion of Metals in non-respirable Gases, by means of Galvanism.**

Metals burned in nitrogen, &c. by galvanism.

PROFESSOR Tromsdorff has noticed that metals are combustible by means of the galvanic spark in hidrogen, ammonia, nitrogen, nitrous and carbonic acid gases.

Reduction of the Oxide of Titanium.

Reduction of titanium.

Professor Lampadius has succeeded in reducing to the metallic state by means of charcoal only, the oxide of titanium, obtained by decomposing the gallate of titanium by potash or soda. The metallic titanium is of a dark copper colour; it

* Tromsdorff's History of Galvanism and its chemical agency, p. 122.

has

has much metallic brilliancy, is brittle, and poſſeſſes in ſmall ſcales a conſiderable degree of elaſticity. It tarniſhes on expoſure to air, and becomes eaſily oxided by heat. It then acquires a blueiſh aſpect. It detonates with nitrate of potaſh, and is highly infuſible. All the denſe acids act upon it with conſiderable energy. *Scherer's Journ.* IX. p. 49. p. 72.

On the Precipitability of the Oxide of Biſmuth.

Mr. Buckholtz has found that the ſolution of biſmuth prepared *in the cold* is alone decompoſable, by a copious addition of water, but that no ſuch effect takes place in the ſolution prepared *by means of heat.* He has alſo noticed that a ſolution of this metal prepared in thec olde, depoſits its oxide in a cryſtalline form merely by warming the ſolution gradually. Solution of biſmuth by heat not precipit. by water.

Scherer IX. p. 73.

New Method of preparing phoſphate of Soda.

Mr. Funcke, apothecary at Linz, in Germany, has diſcovered a new method of preparing phoſphate of ſoda, in a more œconomical, expeditious and eaſy manner, than any of the proceſſes hitherto made uſe of by manufacturers or chemiſts. His proceſs conſiſts, in ſaturating the exceſs of lime contained in calcined bones with dilute ſulphuric acid, and then diſſolving the remaining phoſphate of lime in nitric acid. To this ſolution, he adds a like quantity of ſulphate of ſoda, and then recovers the nitric acid by diſtillation. The phoſphate of ſoda is then ſeparated from the ſulphate of lime, by the affuſion of water, and cryſtallization in the uſual manner. *Scherer* IX. 59. Preparation of phoſphate of ſoda.

REMARK BY THE TRANSLATOR. (A.)

This proceſs ſeems to be much preferable to that now in uſe, *viz.* to decompoſe the bones of animals burned to whiteneſs, by ſulphuric acid, and then preſenting ſoda to the diſengaged phoſphoric acid. For the phoſphate of lime cannot be completely decompoſed by the affuſion of ſulphuric acid; on account of this acid forming inſtantly a portion of ſulphate of lime: the liberated phoſphoric acid then produces with the remaining undecompoſed portion of phoſphate of lime, a ſub-phoſphate of lime, which cannot be decompoſed by ſulphuric acid, and which together with the ſulphate of lime already produced, forms an

unmanageable and bulky mafs. And again if to this mafs, wafhed out with water as well as poffible, carbonate of foda be now prefented, a partial decompofition will only be effected, for it is the excefs of the phofphoric acid of this falt only, which in that cafe forms the article fought. The remaining portion of phofphoric acid remain united to the lime, in the form of phofphate of lime. The above procefs is therefore evidently better.

Sulphate of Soda prepared from Sulphate of Lime.

Sulphate of foda from gypfum.

This method confifts in making into a pafte with a fufficient quantity of water, eight parts of burned gypfum, or fulphate of lime, five of clay, and five of common falt. This mixture is burned in a kiln or other convenient oven, and then ground to powder, diffufed in a fufficient quantity of water, which after being ftrained and evaporated, is fuffered to cryftallize. *Scherer* IX. 61.

A
JOURNAL
OF
NATURAL PHILOSOPHY, CHEMISTRY,
AND
THE ARTS.

OCTOBER, 1803.

ARTICLE I.

Analysis of the Egyptian Heliotropium; a Mineral lately imported from that Country. By FREDERICK ACCUM, *Practical Chemist, and Teacher of Chemistry. Communicated by the Author.*

A PARTICULARLY beautiful species of silicious stone has lately been imported from Egypt, which was stated in the letter of the person who sent it, to possess the peculiar property of reflecting the rays of the sun red, when immersed in water; and when taken out of this fluid to exhibit the figures of the sun and of the moon, when viewed in a particular direction. But as none of the purchasers of this mineral could make out these singular properties, the price fixed on the article was considerably diminished, and the stone sold at a cheap rate. The beautiful green colour which it possesses, and the capability of receiving a high polish, together with the facility of cutting it, has nevertheless rendered it a convenient article for being worked into different objects of fancy and ornament. Beautiful silicious stone of a green colour from Egypt.

The colour of this stone is a fine apple-green. It is very hard, and cannot be scratched, but with the point of a good pen-knife. Its fracture is even and free from all asperities. It breaks with very sharp edges, and its texture is very compact. It is semi-transparent when in pieces not exceeding half an External appearance and properties.

 inch

inch in thicknefs. It ftrikes fire with fteel, and breaks into acute angular irregular pieces. Its fpecific gravity is 2,708. The ftone is fold by the venders by the name of *Egyptian Heliotropium*. Its nature will be more clearly deduced from the following examination:

Analyfis.

Analyfis. Ignition rendered it of a chocolate brown, with white and yellow veins; the polifh uninjured; lofs 1-21ft part.

An entire polifhed piece of the ftone, weighing 250 grains, was expofed to a white heat, in a wind-furnace, for two hours, and then fuffered to cool. The original green colour of the mineral, was changed during this procefs to a chocolate brown, with fnow white and lemon yellow veins. The polifh of the ftone was not injured, but its weight was diminifhed 12 grains.

Diffufion of the powdered ftone in fourparts potafh, fufion, folution of the mafs in water, rather turbid.

Five hundred grains of the ignited ftone were finely pulverized, and mixed with 2000 grains of potafh, diffolved in a like quantity of water. This mixture was evaporated to drynefs in a crucible of platina, and expofed to heat, gradually increafed to whitenefs, for one hour. During this operation the mixture fufed quietly, and exhibited when cold, a homogeneous opake mafs, of a reddifh colour. This mafs was covered with water, and after having been expofed to a gentle heat, a folution of it was effected in that fluid.

Saturated with muriatic acid; gelatinous mafs. Dilution with water.

This alcaline folution was a little turbid, and could not be rendered tranfparent by repeated filtration. I faturated it with muriatic acid, a white precipitate fell down, the whole fluid acquired a reddifh hue, and affumed a gelatinous confiftence. The latter could but difficultly be diminifhed, by a copious admixture of water.

Evaporation nearly to drynefs, addition of dilute muriatic acid; precipitate, *filex*.

In order to collect the precipitate which was diffufed through the fluid, the whole was evaporated nearly to drynefs, and then transferred into muriatic acid, diluted with fix times its quantity of water, and afterwards filtered. The precipitate, after having been wafhed, dried, and ignited, weighed 365 grains. It was pure filex.

Refidual fluid concentrated and faturated with carbonate of potafh.

The fluid from which thefe earths had been feparated, together with the water expended for wafhing it, I concentrated by evaporation to about $\frac{1}{3}$ of its original bulk, and then faturated it with a heated folution of carbonate of potafh in excefs. The white precipitate which was depofited, I transferred into a flafk containing muriatic acid; a brifk effervefcence enfued, but no perfect folution could be effected, by either this, or any other

other acid employed. It was therefore filtered, and the insoluble part collected. This insoluble residue was of a fine red colour, and harsh to the touch. It weighed $2\frac{1}{4}$ grains. But after having been boiled in muriatic acid, it lost $\frac{1}{4}$ grain, and acquired a perfect white colour. The acid made use of for that purpose proved to have stripped it of iron to that amount. It was therefore a portion of silex which had eluded the first process employed for separating this earth.

Insoluble (red) portion boiled with muriatic acid gave a little iron, and left silex.

The muriatic solution from which this siliceous earth had been separated, I mingled with a solution of carbonate of potash, till no further turbidness ensued. The obtained precipitate was transferred after repeated ablutions in water, into a boiling solution of potash, and digested in that fluid for one hour. The mixture was then diluted with water, and the insoluble part separated by the filtre.

The last muriatic solution treated as before.

The alcaline solution was saturated with muriatic acid, and decomposed by carbonate of ammonia in excess; the precipitate obtained by this means, after having acquired a considerable consistence, was digested in acetous acid, and examined for magnesia, but no vestige of such earth could be detected It was therefore dried and ignited, its weight amounted to $20\frac{1}{4}$ grains.

The alcaline solution was decomposed by carbonate of ammonia, and digested in acetous acid, and the solution contained no magnesia, but only alumine.

The insoluble residue from which this earth had been separated, was again dissolved in muriatic acid, and into this solution I dropped liquid ammonia till the odour of the latter considerably predominated. A brown flocculent precipitate fell down, which was collected, washed in liquid ammonia, and boiled for a few minutes in a solution of potash. It was then transferred into a small retort, and nitric acid affused upon it, which was again distilled off from it repeatedly. The retort was lastly heated to a dull redness. The precipitate now weighed 29 grains.

The residue not taken up by the acetous acid was again dissolved in mur. acid, and precip. by excess of ammonia.

This precip. after perfect separation of all acid, and complete oxidation by nitric acid, was weighed as *oxide of iron*.

The fluid from which this precipitate had been obtained, together with the solution of potash made use of for boiling it, was then mixed with muriatic acid, and afterwards decomposed by the addition of carbonate of potash. The product obtained, effervesced with muriatic acid, and yielded sulphate of lime on essaying a small quantity of it by sulphuric acid. It was therefore dried, and exposed to a white heat, after which its weight was $56\frac{1}{2}$ grains.

The fluid from which the iron had been separated was mixed with mur. acid and decomposed by carb. potash. *Lime* fell down.

 The

The refiduary fluid was further examined in the ufual manner, but no other fubftance could be found, but what had been introduced during the different proceffes made ufe of in this examination. The analyfis of the mineral being therefore compleated, from which it appears that 500 grains of the Egyptian heliotropium contain,

Component parts of Egyptian heliotropium.

Silex - - - - - -	365 grains
Alumine - - - - -	$20\frac{1}{4}$
Oxide of Iron - - -	$29\frac{1}{4}$
Lime - - - - -	$56\frac{1}{2}$
Water - - - - -	24.
	495
Lofs	5
	500

Old Compton-Street, Soho,
Sept. 8th, 1803.

P. S. In my paper on the compound of phofphorus and fulphur in your laft Journal (Auguft) I obferve a typographical error, p. 5. l. 22. which materially affects the fenfe, and which I will thank you to correct, namely, for $\frac{1}{3}$, read $\frac{1}{38}$ part.

F. A.

II.

Method of clofing wide mouthed Veffels intended to be kept from communicating with the Air. In a Letter from Anthony Carlisle, Efq.

To Mr. NICHOLSON

Dear Sir,

Clofure of wide mouthed veffels containing anatomical preparations.

It is frequently defirable to clofe the openings of wide-mouthed veffels intended to contain fubftances which would be injured by free expofure to the atmofphere, or to evaporation. The prefent obfervations, however, originate in attempts made to improve the art of preferving anatomical preparations.— The moft ufual liquids employed for what are termed " wet preparations," are weak ardent fpirits, and diftilled oil of turpentine,

pentine, to which may be added an aqueous ſolution of hydrargyrus muriatus, or mercury corroſive ſublimate in the proportion of twenty grains of the latter to a pound of pure water.

The methods ordinarily adopted for cloſing glaſs veſſels uſed for theſe purpoſes, have been ground glaſs ſtoppers, well ſoaked bladders with a middle plate of thick ſheet lead, to keep the top level, and plates of glaſs luted with glaziers putty. Uſual methods.

The objections to theſe methods are found by experience to be ſufficient to induce the trial of others. The ground glaſs ſtoppers are ſeldom air-tight, but when they are, it happens that by the accumulating of particles of duſt in the fitting, the ſtopper becomes in a few years immoveable. Where oil of turpentine is employed, the ſtopper becomes fixed by hardened turpentine. The ſtopping with bladders and ſheet lead is liable to ſuch repeated changes of preſſure within and without, by the alterations in the expanſion of the contained liquids from variations of temperature; that the coheſion of the bladders are eventually deſtroyed; add to this, that ſuch preparations cannot be taken out of the veſſels for examination, or the liquor renewed, without the trouble of a new ſtopping. The plate of glaſs with putty is ſeldom air tight, but when it is ſo, the ſtopping is liable to the ſame objection which was ſtated laſtly againſt the bladders. Objections to each ſeverally.

The method I have now adopted, is to have a glaſs jar with a groove half an inch deep round the outſide of the top or mouth, and a glaſs lid, like that uſed by confectioners in their ſhow glaſſes, the lid fitting looſely into the groove is rendered air tight by hog's lard, a ſubſtance never quite fluid at the higheſt temperature of this climate, and always ſoft enough in the cold ſeaſon to admit of removing the lid or top. New method by a jar, the cover of which fits into a groove with hog's lard.

The firſt glaſs of this kind was made to my order by Mr. Parker in Fleet-ſtreet, to contain twenty ounce meaſures of water, and the coſt was five ſhillings. A ſimilar adjuſtment for the lids of earthen jars, to contain pickles, preſerves, &c. ſeems both eligible and eaſy in practice. See Plate V. Fig. 2.

I am,

DEAR SIR,

Your obliged Friend,

A. CARLISLE.

Soho-Square.

Extract

III.

*Extract of a Letter from Toulon to General le Vavasseur, Inspector of the Materials of the Guns of the French Navy, on the Changes which Cast Iron undergoes by remaining long in the Sea.**

Cast iron lying thirty years at the bottom of the sea was oxided,

Not uniformly, but in veins.

AN observation I have never heard explained is, that cast iron, which has lain a long time at the bottom of the sea, is not equally oxided throughout. I formerly saw a cannon weighed up, after it had been sunk thirty years, which was so much oxided in veins, that I could run a knife into some places, while the metal close by was impenetrable; and on carrying the knife beyond this hard vein, it entered as before. A gun has just been weighed up here (at Toulon,) belonging to one of the ships burnt when the English evacuated the city. The middle is so uniformly oxided, that a large piece may be cut off with a hatchet. Toward the breech, and toward the mouth, the metal appears to have lost nothing of its hardness. Can this difference be ascribed to the contact of the substances in which the gun was buried underneath the water? Its position at the bottom of the sea not being known, we can form no conjectures on this point. For my part I had imagined, from the hard veins of the cannon mentioned above, that its metal had intermixed with it substances on which salt water could not act. The gun lately taken out of the sea appeared more homogenous, but I cannot frame any satisfactory explanation of the fact.†

A cannon sunk in a ship burned at the evacuation of Toulon was oxided only in the middle.

* *Annales de Chimie*, V. 130.

† It is probable, that the last cannon, as it belonged to a ship that was burned, had part of it heated to such a point, when it fell into the sea, as would occasion it to be oxided in a higher degree than the others. It appears to me, more difficult to explain the different veins exhibited by the former cannon. *Note of General le Vavasseur.*

IV.

On the Antiquity of the Invention of Gun-powder, and its firſt Application to Military Purpoſes. By Mr. WIEGLEB.*

THE period of the invention of Gun-powder, and its firſt application for the purpoſe of artillery, has not yet been accurately aſcertained. Though there are many accounts which have been given concerning the invention of this deſtructive compound, yet none of them ſtate their authorities. The uncertainty concerning the invention of gun-powder ſeems merely to be owing to the want of proper documents. The moſt ſummary accounts the author of this paper could find, are contained in two Eſſays, the one written by Gram, the other by Tremler. The firſt account contains the hiſtory of the invention of gun-powder in Europe, and its firſt application in Denmark; the latter comprehends both the invention of powder, and the uſe of guns among the Europeans. Hiſtorical facts to determine the firſt invention of gunpowder, and its uſe in war.

According to the opinion of Gram, gun-powder was already known in Europe about the year 1340. Tremler, on the contrary, endeavours to prove, that no author of credit has poſitively ſhewn that it was known before the year 1354. Theſe authors, therefore, differ only 14 years, reſpecting the period of the invention. Being the other day, ſays Mr. Wiegleb, in ſearch of ſome documents depoſited in the archives of this town (Langenſalze), I happened to meet with the annual account of the expences of the town for the year 1378. This account contains a ſpecification of different arms, viz. pikes, bows, croſs-bows, arrows, guns and gun-powder. The following articles particularly fixed his attention:

1. One gun. 2. One gun and one charge of lead. 3. One gun and one charge of lead. 4. Two guns and two charges of lead. 5. One gun and one charge of lead. 6. Two guns and three charges of lead. Beſides theſe articles, the following charge was made in the account:

Pro Pulveribus, 35 ſhillings.
Pro Pulveribus quos domini emerunt ad Pyxides, 3 ſhillings.
Pro duabus Patellis ad Pyxides, 3 ſhillings.

* From the German of Crell's Annales, v. xx. p. C,

If

Historical facts to determine the first invention of gunpowder, and its use in war.

If we consider that guns, powder, and lead, are here charged, it is obvious that by the word *pulvis*, gun-powder must be understood; and by *patellis*, guns must be meant. From these documents we are led to believe that guns and gun-powder were known already before the year 1378. And it is more than probable that they were not purchased that year, but had probably been used before that time. For guns were too expensive for single individuals and small towns at that time; and on that account, the place in which I met with the said documents was very probably provided with guns many years after the invention of them. That this must have been the case becomes obvious from the following observation: Achilles Gesner, the Historian of Augsburg, who wrote a Latin Chronicle at the beginning of the 16th century, says,* "Three large cannons were cast at Augsburg in the "year 1378, the largest of which discharged a ball of 127 "pounds; the second a ball of 70 pounds, and the third a "ball of 50 pounds, at a distance of 1000 paces."

Herman Corner, who lived at the end of the 14th century, relates that the inhabitants of Lubec assisted the Emperor Charles, who besieged the castle of Dannenberg, with 600 *armatis cum duabus machinis. Bombardae enim pro tunc non erant ita communes, uti nunc sunt:* From hence it is evident that the 600 machines were nothing but bombardae, or guns.

Another remarkable document the author of this paper met with was the sentence of death of Nicolaus the Bold, who supplied the enemy with two barrels of gun-powder, in the year 1372. In this sentence it is clearly expressed, that the gun-powder was made up of saltpetre and sulphur. In the same year, the Corporation of Augsburg ordered to have cast, twenty cannons of metal, at the great expence of fifty pence. These cannons were intended to be used against their neighbours, the Bavarians.

Petrarch, born 1304, states, in his work † published 1374.

GAV. *Habeo machinas, ingentia saxa torquentes.*

RAL. *Saxa torquere furiosum est.*

GAV. *Habeo machinas et ballistas innumeras.*

RAL. *Mirum nisi et glandes Aeneas quae flammis ejectis horrison tone tru jacuntur. Non erat sates de coelo tonantes ira*

* Annales Augsburgenses.

† De Remediis utriusque fortunae.

Dei immortalis, homuncio? O credulitas juncta fuberbiae. De terra etiam tonuiffet: Non immutabile fulmen ut Maro ait humana rabies immitata eft, et quod e nubibus mitti folet, ligneo quidem, fed tartareo mittitur inftrumento. Erat haec peftis nuper rara ut cum ingenti miraculo cerneretur, nunc ut rerum peffimarum dociles funt animi, ita communis eft ut unum quodlibet genus armorum.

Hiftorical facts to determine the firft invention of gunpowder, and its ufe in war.

We fhall be lefs furprifed that cannons and guns were made of wood; even in the 15th century guns were bound with iron hoops.

In the year 1365, Margraff Frederick, of Meiffen, attempted to ftorm the town and caftle of Einben with flings, battering rams, and other machines, then made ufe of in befieging towns. Rothe, who mentions this in his Chronicle of Thuringen, farther relates that the Duke Albert was in poffeffion of a gun which he himfelf ufed at the fiege, for fhooting into the works of the enemy. It was, fay this author, the firft gun ever feen in that country.

In another document is ftated, that *anno Domini, millefimo tricentefimo fexagefimo, confiftorium urbis Lubecenfis in toto combuftum eft, per negligentiem illorum qui pulveras pro bombardis parabant.* The fame fire is mentioned by Herman Corner, a native of Lubeck. His words, as taken from the Chronicle of Lubeck, are as follows: *Confiftorium urbis Lubecenfis incenfum eft, et combuftum per negligentiam illorum qui pulveres pro bombarbis, five petraries parabant, fecundum Chronicam Lubecenfem. Cum enim praedictas paraffent locabant eos in quodam loco confiftoris non caute cuftoditos ab igne. Pulveres ergo per incuriam nocte accenfi, domum ipfam fuccenderunt, ad antequam extingui potuiffent, eum in cineres redegerunt.* Confequently gunpowder muft have been prepared already at Lubeck about the year 1360.

In the year 1359, a war broke out between the kings of Caftile and Arragonia; in which the latter made ufe of a large gun, with which he did much damage to the veffels of the king of Caftile; for he fhot down with it the mafts and rigging, and killed many men by only two fhots.

Peter Divacus commemorates, that in the year 1356 the inhabitants of Lyons in Brabant purchafed 12 guns (bombardae) which were called thundering guns, or blunderbuffes, *ab horrendo fragore.*

V.

A Chemical Analyfis of fome Calamines. By JAMES SMITHSON, Esq. F. R. S. P. R. S. *From the Philofophical Tranfactions for* 1803.

Refearches of Bergman on Calamines.

NOTWITHSTANDING the experiments of Bergman and others, on thofe ores of zinc which are called calamine, much uncertainty ftill fubfifted on the fubject of them. Their conftitution was far from decided, nor was it even determined whether all calamines were of the fame fpecies, or whether there were feveral kinds of them.

Hauy's opinon that they are pure oxides.

The Abbé Hauy, fo juftly celebrated for his great knowledge in cryftallography and mineralogy, has adhered, in his late work *, to the opinions he had before advanced †, that calamines were all of one fpecies, and contained no carbonic acid, being a fimple calx of zinc, attributing the effervefcence which he found fome of them to produce with acids, to an accidental admixture of carbonate of lime.

Experiments.

The following experiments were made to obtain a more certain knowledge of thefe ores; and their refults will fhow the neceffity there was for their farther inveftigation, and how wide from the truth have been the opinions adopted concerning them.

Calamine from Bleyberg.

Calamine from Bleyberg.

a. The fpecimen which furnifhed the fubject of this article, was faid by the German of whom it was purchafed, to have come from the mines of Bleyberg in Carinthia.

External characters.

It was in the form of a fheet ftalactite, fpread over fmall fragments of limeftone. It was not however at all cryftalline, but of the dull earthy appearance of chalk, though, on comparifon, of a finer grain and clofer texture.

It was quite white, perfectly opaque, and adhered to the tongue; 68.0 grs. of it, in fmall bits, immerfed in diftilled water, abforbed 19.8 grs. of it, = 0,29.

It admitted of being fcraped by the nail, though with fome difficulty: fcraped with a knife, it afforded no light.

* *Traité de Mineralogie,* Tome IV. † *Journal des Mines.* No. 32.

68.1 grs.

68.1 grs. of it, broken into ſmall pieces, expelled 19.0 grs. of diſtilled water from a ſtopple bottle. Hence its denſity = 3.584. In another trial, 18.96 grs. at a heat of 65° Fahrenheit, diſplaced 5.27 grs. of diſtilled water; hence the denſity = 3.598. The bits, in both caſes, were entirely penetrated with water. Specific gravity.

b. Subjected to the action of the blowpipe on the coal, it became yellow the moment it was heated, but recovered its priſtine whiteneſs on being let cool. This quality, of temporarily changing their colour by heat, is common to moſt, if not all, metallic oxides; the white growing yellow, the yellow red, the red black. Blowpipe aſſays.

Urged with the blue flame, it became extremely friable; ſpread yellow flowers on the coal; and, on continuing the fire no very long time, entirely exhaled. If the flame was directed againſt the flowers, which had ſettled on the coal, they ſhone with a vivid light. A bit fixed to the end of a ſlip of glaſs, waſted nearly as quickly as on the coal.

It diſſolved in borax and microcoſmic ſalt, with a ſlight effervefcence, and yielded clear colourleſs glaſſes; but which became opaque on cooling, if over ſaturated. Carbonate of ſoda had not any action on it.

c. 68.0 grs. of this calamine diſſolved in dilute vitriolic acid with a briſk effervefcence, and emitted 9.2 grs. of carbonic acid. The ſolution was white and turbid, and on ſtanding depoſited a white powder, which, collected on a ſmall filter of gauze paper, and well edulcorated and let dry, weighed only 0.86 grs. This ſediment, tried at the blowpipe, melted firſt into an opaque white matter, and then partially reduced into lead. It was therefore, probably, a mixture of vitriol of lead and vitriol of lime. Solutions in the ſulphuric and muriatic acids gave only ſalts of zinc, and gave carbonic acid.

The filtered ſolution, gently exhaled to dryneſs, and kept over a ſpirit-lamp till the water of cryſtallization of the ſalt and all ſuperfluous vitriolic acid were driven off, afforded 96.7 grs. of perfectly dry, or *arid* *, white ſalt. On re-ſolution in water, and cryſtallization, this ſaline matter proved to be wholly vitriol

* *Dry*, as oppoſed to wet or damp, which are only degrees of each other, merely implies free from mechanically admixed water. *Arid*, may be appropriated to expreſs the ſtate of being devoid of combined water.

of

of zinc, excepting an inappretiable quantity of vitriol of lime in capillary cryſtals, due, without doubt, to a ſlight and accidental admixture of ſome portion of the calcareous fragments on which this calamine had been depoſited. Pure martial pruſſiate of tartar, threw down a white precipitate from the ſolution of this ſalt.

In another experiment, 20.0 grs. of this calamine afforded 28.7 grs. of arid vitriol of zinc.

d. 10 grs. of this calamine were diſſolved in pure marine acid, with heat. On cooling, ſmall capillary cryſtals of muriate of lead formed in the ſolution. This ſolution was precipitated by carbonate of ſoda, and the filtered liquor let exhale ſlowly in the air; but it furniſhed only cryſtals of muriate of ſoda.

e. 10 grs. diſſolved in acetous acid without leaving any reſiduum. By gentle evaporation, 20.3 grs. = 2.03. of acetite of zinc, in the uſual hexagonal plates, were obtained. Theſe cryſtals were permanent in the air, and no other kind of ſalt could be perceived amongſt them.

Neither ſolution of vitriolated tartar, nor vitriolic acid, occaſioned the ſlighteſt turbidneſs in the ſolution of theſe cryſtals, either immediately or on ſtanding; a proof that the quantity of lime and lead in this ſolution, if any, was exceſſively minute.

f. A bit of this calamine, weighing 20.6 grs. being made red hot in a covered tobacco-pipe, became very brittle, dividing on the ſlighteſt touch into priſms, like thoſe of ſtarch, and loſt 5.9 grs. of its weight = 0.286. After this, it diſſolved ſlowly and difficultly in vitriolic acid, without any efferveſcence.

The calamine contained oxide of zinc, carbonic acid, and water.

According to theſe experiments, this calamine conſiſts of,

Calx of zinc - - -	0.714
Carbonic acid - - - -	0.135
Water - - - - -	0.151
	1.000.

The carbonates of lime and lead in it are more accidental admixtures, and in too ſmall quantity to deſerve notice.

Calamine from Somerſetſhire.

Somerſetſhire calamine.

a. This calamine came from Mendip Hills in Somerſetſhire. It had a mammillated form; was of a denſe cryſtalline texture; ſemitranſparent at its edges, and in its ſmall fragments; and upon the whole very ſimilar, in its general appearance, to calcedony.

It

It was tinged, exteriorly, brown; but its interior colour was a greenifh yellow.

It had confiderable hardnefs; it admitted however of being fcraped by a knife to a white powder. Characters.

56.8 grs. of it difplaced 13.1 grs. of water, at a temperature of 65° Fahrenheit. Hence its denfity = 4.336.

b. Expofed to the blowpipe, it became opaque, more yellow, and friable; fpread flowers on the coal, and confequently volatilized, but not with the rapidity of the foregoing kind from Bleyberg. Blowpipe affays.

It diffolved in borax and microcofmic falt, with effervefcence, yielding colourlefs glaffes. Carbonate of foda had no action on it.

c. It diffolved in vitriolic acid with a brifk effervefcence; and 67.9 grs. of it emitted 24.5 grs. = 0.360, of carbonic acid. This folution was colourlefs; and no refiduum was left. By evaporation, it afforded only vitriol of zinc, in pure limpid cryftals. Solution.

d. 23.0 grs. in fmall bits, made red hot in a covered tobacco-pipe, loft 8.1 grs. = 0.352. It then diffolved flowly and difficultly in vitriolic acid, without any emiffion of carbonic acid; and, on gently exhaling the folution, and heating the falt obtained, till the expulfion of all fuperabundant vitriolic acid and all water, 29.8 grs. of arid vitriol of zinc were obtained. This dry falt was wholly foluble again in water; and folution of pure martial pruffiate of foda occafioned a white precipitate in it.

This calamine hence confifts of, Component parts, Carbonic acid and oxide of zinc.

Carbonic acid - - -	0.352
Calx of zinc - - - -	0.648
	1.000.

Calamine from Derbyfhire.

a. This calamine confifted of a number of fmall cryftals, about the fize of tobacco-feeds, of a pale yellow colour, which appeared from the fhape of the mafs of them, to have been depofited on the furface of cryftals of carbonate of lime, of the form of Fig. 28. Plate IV. of the *Criftallographie* of Romé de L'Ifle. Derbyfhire calamine.

The fmallnefs of thefe calamine cryftals, and a want of fharpnefs rendered it impoffible to determine their form with certainty: External characters, &c.

certainty; they were evidently, however, rhomboids, whofe faces were very nearly, if not quite, rectangular, and which were incomplete along their fix intermediate edges, apparently like Fig. 78. Plate IV. of Romé de L'Ifle.

22.1 grs. of thefe cryftals, at a heat of 57° Fahrenheit, difplaced 5.1 grs. of water, which gives their denfity = 4 333.

Heat did not excite any electricity in thefe cryftals.

Experiments.

b. Before the blowpipe, they grew more yellow and opaque, and fpread flowers on the coal. They diffolved wholly in borax and microcofmic falt, with effervefcence.

c. 22.0 grs. during their folution in vitriolic acid, effervefced, and loft 7.8 grs. of carbonic acid = 0.354. This folution was colourlefs, and afforded 26.8 grs. of arid vitriol of zinc, which, rediffolved in water, fhot wholly into clear colourlefs prifms of this falt.

Component parts carbonic acid and oxide of zinc.

d. 9.2 grs. of thefe cryftals, ignited in a covered tobacco-pipe, loft 3.2 grs. = 0.3478; hence thefe cryftals confift of,

Carbonic acid	- - -	0.348
Calx of zinc	- - - -	0.652
		1.000.

Electrical Calamine.

Electrical calamine from Regbania.

The Abbé Hauy has confidered this kind as differing from the other calamines only in the circumftance of being in diftinct cryftals; but it has already appeared, in the inftance of the Derbyfhire calamine, that all cryftals of calamine are not electric by heat, and hence, that it is not merely to being in this ftate that this fpecies owes the above quality. And the following experiments, on fome cryftals of electric calamine from Regbania in Hungary, can leave no doubt of its being a combination of calx of zinc with quartz; fince the quantity of quartz obtained, and the perfect regularity and tranfparency of thefe cryftals make it impoffible to fuppofe it a foreign admixture in them.

a. 23.45 grs. of thefe Regbania cayftals, difplaced 6.8 grs. of diftilled water, from a ftopple-bottle, at the temperature of 64° Fahrenheit; their fpecific gravity is therefore = 3.434.

The form of thefe cryftals is reprefented in Figure I. Plate V. where the angle formed by the planes *a* and *c* was 90°, that by *a* and *e* = 150°, that by *b* and *c* = 115°, and that by *c* and *d* = 130°.

They were not fcratched by a pin; a knife marked them.

 b. One

b. One of thefe cryftals, expofed to the flame of the blow-pipe, decrepitated and became opaque, and fhone with a green light, but feemed totally infufible. Blowpipe experiments.

Borax and microcofmic falt diffolved thefe cryftals, without any effervefcence, producing clear colourlefs glaffes, Carbonate of foda had little if any action on them.

c. According to Mr. Pelletier's experiments * on the calamine of Fribourg in Brifgaw, which is undoubtedly of this fpecies, its compofition is,

Quartz - - - - - -	0.50
Calx of zinc - - - -	0.38
Water - - - - - -	0.12
	1.00.

The experiments on the Regbania cryftals have had different refults; but, though made on much fmaller quantities, they will perhaps not be found, on repetition, lefs in conformity with nature.

23.45 grs. heated red hot in a covered crucible, decrepitated a little, and became opaque, and loft 1.05 grs. but did not fall to powder or grow friable. It was found, that this matter was not in the leaft deprived of its electrical quality by being ignited; and hence, while hot, the fragments of thefe decrepitated cryftals clung together, and to the crucible.

d. 22.2 grs. of thefe decripitated cryftals, = 23.24 grs. of the original cryftals, in a ftate of impalpable powder, being digefted over a fpirit-lamp with diluted vitriolic acid, fhowed no effervefcence; and, after fome time, the mixture became a jelly. Exhaled to drynefs, and ignited flightly, to expel the fuperfluous vitriolic acid, the mafs weighed 37.5 grs.

On extraction of the faline part by diftilled water, a fine powder remained, which, after ignition, weighed 5.8 grs. and was quartz.

The faline folution afforded, on cryftallization, only vitriol of zinc. Thefe cryftals therefore confift of, Component parts of electrical calamine.

Quartz - - - - -	0.250
Calx of zinc - - - - -	0.683
Water - - - - - -	0.044
	0.977
Lofs - - - - - -	0.023
	1.000.

* Journal de Phyfique. Tom. XX. p. 414.

The

The water is moſt probably not an eſſential element of this calamine, or in it in the ſtate of, what is improperly called, water of cryſtallization, but rather exiſts in the cryſtals in fluid drops interpoſed between their plates, as it often is in cryſtals of nitre, of quartz, &c. Its ſmall quantity, and the cryſtals not falling to powder on its expulſion, but retaining almoſt perfectly their original ſolidity, and ſpathoſe appearance in the places of fracture, and, above all, preſerving their electrical quality wholly unimpaired, which would hardly be the caſe after the loſs of a real element of their conſtitution, ſeem to warrant this opinion.

If the water is only accidental in this calamine, its compoſition, from the above experiments, will be,

Quartz - - - - -	0.261
Calx of zinc - - -	0.739
	1.000

It is found in Derbyſhire.

I have found this ſpecies of calamine amongſt the productions of Derbyſhire, in ſmall brown cryſtals, depoſited, together with the foregoing ſmall cryſtals of carbonate of zinc, on cryſtals of carbonate of lime. Their form ſeems, as far as their minuteneſs and compreſſion together would allow of judging, nearly or quite the ſame as that of thoſe from Regbania; and the leaſt atom of them immediately evinces its nature, on being heated, by the ſtrong electricity it acquires. On their ſolution in acids, they leave quartz.

OBSERVATIONS.

Chemiſtry is yet ſo new a ſcience, what we know of it bears ſo ſmall a proportion to what we are ignorant of, our knowledge in every department of it is ſo incomplete, ſo broken, conſiſting ſo entirely of iſolated points thinly ſcattered like lucid ſpecks on a vaſt field of darkneſs, that no reſearches can be undertaken without producing ſome facts, leading to ſome conſequences, which extend beyond the boundaries of their immediate object.

Component parts of ſulphate of zinc.

1. The foregoing experiments throw light on the proportions in which its elements exiſt in vitriol of zinc. 23.0 grs. of the Mendip Hill calamine, produced 29.8 grs. of arid vitriol of zinc. Theſe 23.0 grs. of calamine contained 14.9 grs. of calx

calk of zinc; hence, this metallic falt, in an arid ftate, confifts of *exactly equal* parts of calx of zinc and vitriolic acid.

This inference is corroborated by the refults of the other experiments: 68.0 grs. of the Bleyberg calamine, containing 48.6 grs. of calx of zinc, yielded 96.7 grs. of arid vitriol of zinc; and, in another trial, 20.0 grs. of this ore, containing 14.2 grs. of calx of zinc, produced 28.7 grs. of arid vitriol of zinc. The mean of thefe two cafes, is 62.7 grs. of arid vitriol of zinc, from 31.4 grs. of calx of zinc.

In the experiment with the cryftals of carbonate of zinc from Derbyfhire, 14.35 grs. of calx of zinc furnifhed indeed only 26.8 grs of arid vitriol of zinc; a deficiency of about $\frac{6}{100}$, occafioned probably by fome fmall inaccuracy of manipulation.

Pofition that the parts of compounds do not greatly exceed each other in quantity.

2. When the fimplicity found in all thofe parts of nature which are fufficiently known to difcover it is confidered, it appears improbable that the proximate conftituent parts of bodies fhould be united in them, in the very remote relations to each other in which analyfes generally indicate them; and, an attention to the fubject has led me to the opinion that fuch is in fact not the cafe, but that, on the contrary, they are univerfally, as appears here with refpect to arid vitriol of zinc, fractions of the compound of very low denominators. Poffibly in few cafes exceeding five.

The fuccefs which has appeared to attend fome attempts to apply this theory, and amongft others, to the compofitions of fome of the fubftances above analyfed, and efpecially to the calamine from Bleyberg, induces me to venture to dwell here a little on this fubject, and ftate the compofition of this calamine, which refults from the fyftem, as, befides contributing perhaps to throw fome light on the true nature of this ore, it may be the means likewife of prefenting the theory under circumftances of agreement with experiment, which, from the furprifing degree of nearnefs, and the trying complexity of the cafe, may feem to entitle it to fome attention.

Hence the component parts of Bleyberg calamine are fuppofed to be arranged in fubordinate compounds.

From this calamine, containing, according to the refults of the experiments on the Mendip Hill kind, too fmall a quantity of carbonic acid to faturate the whole of the calx of zinc in it, and from its containing much too large a portion of water to be in it in the ftate of mere moifture or dampnefs, it feems to confift of two matters; carbonic of zinc, and a peculiar compound of zinc and water, which may be named *hydrate of zinc*.

By the results of the analysis of the Mendip-Hill calamine, corrected by the theory, carbonate of zinc appears to consist of,

Carbonic acid - - - - - . $\frac{1}{3}$
Calx of zinc - - - - $\frac{2}{3}$

Deducting from the calx of zinc in the Bleyberg calamine, that portion which corresponds, on these principles, to its yield of carbonic acid, the remaining quantity of calx of zinc and water are in such proportions as to lead, from the theory, to consider hydrate of zinc as composed of

Calx of zinc - - - - - $\frac{3}{4}$
Water, or rather ice - - - $\frac{1}{4}$

And, from these results, corrected by the theory, I consider Bleyberg calamine as consisting of,

Carbonic of zinc - - - $\frac{2}{5}$
Hydrate of zinc - - - $\frac{3}{5}$

The test of this hypothesis is in the quantities of the remote elements whioh analysis would obtain from a calamine thus composed.

The following table will show how very insignificantly the calamine compounded by the theory, would differ in this respect from the calamine of nature.

Elucidations of chemical theory.

1000 parts of the compound salt of carbonate and hydrate of zinc consist of,

$$
\begin{array}{lll}
\text{Carbonate of zinc } 400 \text{ - - - - } = \left\{ \begin{array}{l} \text{Carbonic acid} = \dfrac{400}{3} = \text{ - - - - - - } \\[2ex] \text{Calx of zinc} = \dfrac{400 \times 2}{3} = 266\tfrac{2}{3} \end{array} \right. & & \begin{array}{r} 133\tfrac{1}{3} \\[2ex] \\ \end{array} \\[6ex]
\text{Hydrate of zinc} = 600 \text{ - - - - } \left\{ \begin{array}{l} \text{Calx of zinc} = \dfrac{600 \times 3}{4} = 450 \\[2ex] \text{Ice - - } = \dfrac{600}{4} = \text{ - - - - - - } \end{array} \right. & \left.\begin{array}{l} \\ \end{array}\right\} = - 716\tfrac{2}{3} & \begin{array}{r} \\[2ex] 150 \end{array} \\
 & & \overline{1000.}
\end{array}
$$

Great as is the agreement between the quantities of the last column and those obtained by the analysis of the Bleyberg calamine, it would be yet more perfect, probably, had there been, in this instance, no sources of fallacy but those attached to chemical operations, such as errors of weighing, waste, &c. but

but the differences which exift are owing, in fome meafure at leaft, to the admixture of carbonate of lime and carbonate of lead, in the calamine analyfed, and alfo to fome portion of water, which is undoubtedly contained, in the ftate of moifture, in fo porus and bibulous a body.

Elucidations of chemical theory.

It has alfo appeared, in the experiments on the Mendip Hill calamine, that acids indicate a greater quantity of carbonic acid than fire does, $\frac{22}{1000}$. If we make this deduction for diffolved water, it reduces the quantity of carbonic acid in the Bleyberg calamine, to 0.1321.

If we affume this quantity of carbonic acid as the datum to calculate, on this fyftem, the compofition of the calamine from Bleyberg, we fhall obtain the following refults:

Compound falt, of carbonate of zinc and hydrate of zinc	990.3
Water in the ftate of moifture - - - -	2.5
Carbonate of lime and carbonate of lead - -	7.2
	1000.0

It may be thought fome corroboration of the fyftem here offered, that, if we admit the proportions which it indicates, the remote elements of this ore, while they are regular parts of their immediate products, by whofe fubfequent union this ore is engendered, are alfo regular fractions of the ore itfelf: thus,

The carbonic acid - - - $=\frac{8}{60}$
The water - - - - $=\frac{9}{60}$
The calx of zinc - - - - $=\frac{43}{60}$

Hereby difplaying that fort of regularity, in every point of view of the object, which fo wonderfully characterifes the works of nature, when beheld in their true light.

If this calamine does confift of carbonate of zinc and hydrate of zinc, in the regular proportions above fuppofed, little doubt can exift of its being a true chemical combination of thefe two matters, and not merely a mechanical mixture of them in a pulverulent ftate; and, if fo, we may indulge the hope of fome day meeting with this ore in regular cryftals.

If the theory here advanced has any foundation in truth, the difcovery will introduce a degree of rigorous accuracy and certainty into chemiftry, of which this fcience was thought to be ever incapable, by enabling the chemift, like the geometrician, to rectify by calculation the unavoidable errors of his

 manual

Elucidations of chemical theory.

manual operations, and by authoriſing him to eliminate from the eſſential elements of a compound, thoſe products of its analyſis whoſe quantity cannot be reduced to any admiſſible proportion.

A certain knowledge of the exact proportions of the conſtituent principles of bodies, may likewiſe open to our view harmonious analogies between the conſtitutions of related objects, general laws, &c. which at preſent totally eſcape us. In ſhort, if it is founded in truth, its enabling the application of mathematics to chemiſtry, cannot but be productive of material reſults *.

3. By the application of the foregoing theory to the experiments on the electrical calamine, its elements will appear to be,

Quartz - - - - - -	$\frac{1}{4}$
Calx of zinc - - - - - -	$\frac{3}{4}$

A ſmall quantity of the calamine having eſcaped the action of the vitriolic acid, and remained undecompoſed, will account for the ſlight exceſs in the weight of the quartz.

4. The exhalation of theſe calamines at the blowpipe, and the flowers which they diffuſe round them on the coal, are probably not to be attributed to a direct volatilization of them. It is more probable that they are the conſequences of the diſoxidation of the zinc calx, by the coal and the inflammable matter of the flame, its ſublimation in a metallic ſtate, and inſtantaneous recalcination. And this alternate reduction and combuſtion, may explain the peculiar phoſphoric appearance exhibited by calces of zinc at the blowpipe.

The apparent ſublimation of the common flowers of zinc at the inſtant of their production, though totally unſublimable afterwards, is certainly likewiſe but a deceptious appearance. The reguline zinc, vaporiſed by the heat, riſes from the crucible as a metallic gas, and is, while in this ſtate, converted to a calx. The flame which attends the proceſs is a proof of it; for flame is a maſs of vapour, ignited by the production of fire within itſelf. The fibrous form of the flowers of zinc, is owing to a cryſtallization of the calx while in *mechanical ſuſ-*

* It may be proper to ſay, that the experiments have been ſtated *preciſely* as they turned out, and have not been in the *leaſt degree* bent to the ſyſtem.

penſion

penſion in the air like that which takes place with camphor when, after having been ſome time inflamed, it is blown out. Elucidations of chemical theory.

A moment's reflection muſt evince, how injudicious is the common opinion, of cryſtallization requiring a ſtate of ſolution in the matter; ſince it muſt be evident, that while ſolution ſubſiſts, as long as a quantity of fluid admitting of it is preſent, no cryſtallization can take place. The only requiſite for this operation, is a freedom of motion in the maſſes which tend to unite, which allows them to yield to the impulſe which propels them together, and to obey that ſort of polarity which occaſions them to preſent to each other the parts adapted to mutual union. No ſtate ſo completely affords theſe conditions as that of mechanical ſuſpenſion in a fluid whoſe denſity is ſo great, relatively to their ſize, as to oppoſe ſuch reſiſtance to their deſcent in it as to occaſion their mutual attraction to become a power ſuperior to their force of gravitation. It is in theſe circumſtances that the atoms of matters find themſelves, when, on the ſeparation from them of the portion of fluid by which they were diſſolved, they are abandoned in a diſengaged ſtate in the boſom of a ſolution; and hence it is in ſaturated ſolutions ſuſtaining evaporation, or equivalent cooling, and free from any perturbing motion, that regular cryſtallization is uſually effected.

But thoſe who are familiar with chemical operations, know the ſort of agglutination which happens between the particles of ſubſided very fine precipitates: occaſioning them, on a ſecond diffuſion through the fluid, to ſettle again much more quickly than before, and which is certainly a cryſtallization, but under circumſtances very unfavourable to its perfect performance.

5. No calamine has yet occurred to me which was a real, uncombined, calx of zinc. If ſuch, as a native product, ſhould ever be met with in any of the ſtill unexplored parts of the earth, or exiſt amongſt the unſcrutinized poſſeſſions of any cabinet, it will eaſily be known, by producing a quantity of arid vitriol of zinc exactly double its own weight; while the hydrate of zinc, ſhould it be found ſingle, or uncombined with the carbonate, will yield, it is evident, 1.5 its weight of this arid ſalt.

VI.

Table of the Radii of Wheels, from Ten to Three Hundred Teeth, the Pitch being Two Inches.* By Mr. B. DONKIN, *Millwright, Dartford, Kent* †.

Table of the radii of wheels.

No. of Teeth	Radius in Inches.	No.	Radius.	No.	Radius.
10	3,236	42	13,382	74	23,562
11	3,549	43	13,700	75	23,880
12	3,864	44	14,018	76	24,198
13	4,179	45	14,336	77	24,517
14	4,494	46	14,654	78	24,835
15	4,810	47	14,972	79	25,153
16	5,126	48	15,290	80	25,471
17	5,442	49	15,608	81	25,790
18	5,759	50	15,926	82	26,108
19	6,076	51	16,244	83	26,426
20	6,392	52	16,562	84	26,744
21	6,710	53	16,880	85	27,063
22	7,027	54	17,198	86	27,381
23	7,344	55	17,517	87	27,699
24	7,661	56	17,835	88	28,017
25	7,979	57	18,153	89	28,336
26	8,296	58	18,471	90	28,654
27	8,614	59	18,789	91	28,972
28	8,931	60	19,107	92	29,290
29	9,249	61	19,425	93	29,608
30	9,567	62	19,744	94	29,927
31	9,885	63	20,062	95	30,245
32	10,202	64	20,380	96	30,563
33	10,520	65	20,698	97	30,881
34	10,838	66	21,016	98	31,200
35	11,156	67	21,335	99	31,518
36	11,474	68	21,653	100	31,836
37	11,792	69	21,971	101	32,155
38	12,110	70	22,289	102	32,473
39	12,428	71	22,607	103	32,791
40	12,746	72	22,926	104	33,109
41	13,064	73	23,244	105	33,427

* By the pitch is underſtood the diſtance between the centers of two contiguous teeth; and by the radius is underſtood the diſtance between the center of the wheel and the center of each tooth.

† Communicated by the author.

No

Table of the radii of wheels.

No.	Radius.	No.	Radius.	No.	Radius.
106	33,746	151	48,068	196	62,392
107	34,064	152	48,387	197	62,710
108	34,382	153	48,705	198	63,028
109	34,700	154	49,023	199	63,346
110	35,018	155	49,341	200	63,665
111	35,337	156	49,660	201	63,983
112	35,655	157	49,978	202	64,301
113	35,974	158	50,296	203	64,620
114	36,292	159	50,615	204	64,938
115	36,611	160	50,933	205	65,256
116	36,929	161	51,251	206	65,574
117	37,247	162	51,569	207	65,893
118	37,565	163	51,888	208	66,211
119	37,883	164	52,206	209	66,529
120	38,202	165	52,524	210	66,848
121	38,520	166	52,843	211	67,166
122	38,838	167	53,161	212	67,484
123	39,156	168	53,479	213	67,803
124	39,475	169	53,798	214	68,121
125	39,793	170	54,116	215	68,439
126	40,111	171	54,434	216	68,757
127	40,429	172	51,752	217	69,075
128	40,748	173	55,071	218	69,394
129	41,066	174	55,389	219	69,712
130	41,384	175	55,707	220	70,031
131	41,703	176	56,026	221	70,349
132	42,021	177	56,344	222	70,667
133	42,339	178	56,662	223	70,985
134	42,657	179	56,980	224	71,304
135	42,976	180	57,299	225	71,622
136	43,294	181	57,617	226	71,941
137	43,612	182	57,935	227	72,258
138	43,931	183	58,253	228	72,577
139	44,249	184	58,572	229	72,895
140	44,567	185	58,890	230	73,214
141	44,885	186	59,209	231	73,532
142	45,204	187	59,527	232	73,850
143	45,522	188	59,845	233	74,168
144	45,840	189	60,163	234	74,487
145	46,158	190	60,482	235	74,805
146	46,477	191	60,800	236	75,123
147	46,795	192	61,118	237	75,441
148	47,113	193	61,436	238	75,760
149	47,432	194	61,755	289	76,078
150	47,750	195	62,073	240	76,397

No.

Table of the radii of wheels.

No.	Radius.	No.	Radius.	No.	Radius.
241	76,715	261	83,081	281	89,447
242	77,033	262	83,399	282	89,765
243	77,351	263	83,717	283	90,084
244	77,670	264	84,036	284	90,402
245	77,988	265	84,354	285	90,720
246	78,306	266	84,673	286	91,038
247	78,625	267	84,991	287	91,357
248	78,943	268	85,309	288	91,675
249	79,261	269	85,627	289	91,993
250	79,580	270	85,946	290	92,312
251	79,898	271	86,264	291	92,630
252	80,216	272	86,582	292	92,948
253	80,534	273	86,900	293	93,267
254	80,853	274	87,219	294	93,585
255	81,171	275	87,537	295	93,903
256	81,489	276	87,855	296	94,222
257	81,808	277	88,174	297	94,540
258	82,126	278	88,492	298	94,858
259	82,444	279	88,810	299	95,177
260	82,763	280	89,129	300	95,495

N. B. When the pitch is different from two inches, the radius of a wheel of any number of teeth, from 10 to 300 may be found from this table, by the Rule of Three; for as two inches (the pitch in the table) is to any radius in the table, fo is any given pitch to the radius required.

For Example; let it be required to find the radius of a wheel of 100 teeth, when the pitch is $1\frac{1}{4}$ inches. The radius of a wheel of 100 teeth is, in the table, 31,836 inches. Accordingly we have 2 : 31,836 : : 1,25, to the number of inches in the radius required; which will be found 19,897 as by the Operation annexed.

$$\begin{array}{r} 2 : 31{,}836 : : 1{,}25 \\ 1{,}25 \\ \hline 159180 \\ 63672 \\ 31836 \\ \hline 2)\ 39{,}79500 \\ \hline 19{,}8975 \end{array}$$

VII. *Account*

VII.

Account of the Pyrometer of Platina. *By* CITIZEN GUYTON.*

CITIZEN GUYTON prefented an inftrument to the fitting of the French National Inftitute of the 26th Floreal laft, intended to meafure the higheft degrees of heat of our furnaces.

Pyrometer for red heats, confifting of a lever of platina moved by the expanfion of a bar of the fame metal; the whole being fupported on a mafs of baked clay.

It confifts of a rod or plate of platina placed horizontally in a groove formed in a cake of hardened white clay. This plate is fupported at one of its extremities on the part of the mafs which terminates the groove; the other end preffes againft a bended lever, whofe longeft arm forms an index to a graduated arc; fo that the change of pofition of this index indicates the expanfion produced on the plate of metal by the heat.

It is governed by the difference of the expanfions of pottery and platina.

Defcription.

The cake of clay having been highly baked, leaves no caufe to apprehend any contraction; and the expanfion which may take place during the ignition will only affect the very fmall diftance between the axis of motion of the index and the point of contact of the plate, that is to fay, in fuch a manner as rather to diminifh the effect than to increafe it.

All the parts of this inftrument being of platina, neither fufion nor oxidation are to be apprehended.

Dimenfions.

With refpect to its dimenfions, the author conceives that in order to render the ufe of it commodious and accurate, they fhould be reduced to fuch as may be neceffary to obtain fenfible variations; it will then be rendered commodious by the facility with which it may be placed under a muffle or an inverted crucible, &c. and accurate, becaufe the probabilities of any accidental inequalities of the heat will be diminifhed, which it is impoffible to avoid to a certain extent, even in the midft of a large mafs of fire.

Degree of accuracy.

The variations will be fufficiently perceptible, if we can not only eftimate, but correctly determine expanfions of the 200th part of a millimetre (about the 5000th part of an inch,) thefe the author obtains by the proportions of the inftrument which he has himfelf adopted.

Dimenfions of the parts.

The rod or plate of expanfion is 45 millimetres (one inch and three quarters) in length, 5 in width (one fifth of an inch) and 2 in thicknefs (one thirteenth of an inch.)

* *Annales de Chimie,* No. 138. XLVI. 276.

The

The arm of the bended lever, which preſſes againſt the end of this rod, is 25 millimetres in length; (rather $2\frac{1}{2}$, or about one ninth of an inch) and the arm at right angles to it, or the index, which traverſes on the graduated arc, is 50 millimetres in length (one inch and eight tenths) or twenty times the length of the other. The ſpace traverſed by the diſplacing of the ſmall arm will be thus encreaſed in the proportion of 1 to 20.

As the long arm or index carries a nonius which divides each degree on the graduated arc into ten parts, we can diſtinctly obſerve the 200th of one of thoſe meaſures (referred to the bar itſelf.)

Laſtly, As the decimal diviſion of an arc of a circle of 50 millimetres radius, gives only 7.8538 deci-millimetres for one of its degrees, it is evident then that we may meaſure an expanſion of 0.078538 deci-millimetres, or of the 5730th part of the length of the radius.

In order to prevent the poſition of the index from being changed in removing the inſtrument from the furnace, a plate of platina is fixed ſo as to form a ſpring againſt its extremity.

The author has commenced a ſeries of experiments to determine the range of this pyrometer, to compare it with the pyrometric pieces of Wedgewood, and ſo to ſhew the degree of confidence it merits, the methods of uſing, and the caſes in which it may be uſefully employed in philoſophical reſearches and in the arts.

VIII.

Letter from Mr. Ezekiel Walker on the Proportion of Light afforded by Candles of different Dimenſions.

To Mr. NICHOLSON,

SIR,

Obſervations on a letter to the editor.

YOUR correſpondent, who has made ſome remarks on my experiments on candles, does not ſeem to have ſufficiently conſidered his ſubject; for had he paid attention to my paper on page 40 of the fourth volume of your Journal, he could not have advanced that " Though Mr. Walker aſſerts with conſiderable deciſion, that the light afforded by candles, is proportioned to the quantity of material conſumed, yet he has not given

given the detail of his experiments, but seems in some measure to have discovered this result by argument, from the supposed nature of the subject." *

In the table in my paper above mentioned, the last column contains the distances of the candles from the wall, when the shadows were equal; and the fourth column contains the weights of those candles consumed in a given time, and these are all the data required for making the calculations, to show whether my deduction is true or false. The mode of calculating seemed to me, at the time I wrote that paper, too easy to need any illustration, but as I now stand charged by your correspondent, of having deduced a general law from doubtful principles, a further explanation becomes necessary.

Observations of light emitted by candles, and of the weights consumed.

To investigate rules for this purpose, 1. Let M represent the mould candle, a its distance from the wall, on which the shadows were compared, x its quantity of matter consumed in a given time, (t) and Q the quantity of light emitted by M in the same time: 2. Let m represent any other candle, b its distance from the same wall, and y its quantity of matter consumed, in the time t.

Investigation of the rules for computing.

Then as the intensities of light are directly as the squares of the distances of the two candles from the wall, we have, as $a^2 : Q :: b^2 : \frac{b^2 \times Q}{a^2} =$ the quantity of light, emitted by m in the time.

Then let us suppose that the quantities of light are directly as the quantities of matter consumed in the time t, and we have, As $x : Q :: y : \frac{y \times Q}{x} =$ the quantity of light emitted by m in that time, by hypothesis.

Now, when $\frac{b^2 \times Q}{a^2}$ (Theo. 1.) is $= \frac{Y \times Q}{X}$ (Theo. 2.) the quantities of light of M and m are directly as their quantities of matter consumed in any given time.

By these rules, the calculations contained in the following table, were made from the experiments mentioned at the beginning of this paper.

* See Philosophical Journal, VOL. V. page 219.

No.

No of experiments.		Light by Rule I.	Light by Rule II.	2d Rule differs from the 1ft.
1	No. 1. compd. with the mould	1.000	1.000	.000
2	No. 1. compd. with do.	1.000	1.000	.000
3	No. 1. compd. with do.	1.000	1.015	+ .015
3	No. 3. compd. with do.	1.196	1.125	− .071
4	No. 4. compd. with do.	1.196	1.226	+ .030
	The mean error of the 2d Rule			− .005

As the mean refult given by the 2d rule, differs only 1 in 200 from the 1ft, which is univerfally received as true, the 2d rule appears fufficiently exact for many practical purpofes, where the properties of that light is concerned, which is produced by candles.

EZEKIEL WALKER.

Lynn Regis, 20th Sept. 1803.

Whether the experiments of a correfpondent be accurate.

P. S. As to your correfpondent's experiment, it does not appear fo correct to me, as it appears to himfelf; for every one knows, that one end of a mould candle is thicker than the other, therefore if that gentleman made his experiment with the fmall end of his candle, he has eftimated the quantity of light produced by a pound, too little; and if he made his experiment with the large end, his eftimation is too great; and moreover, it may be doubted, whether the $\frac{1}{16}$ part of a pound of candles, can be fo exactly afcertained by meafuring as by weighing, even if the candles were perfect cylnders.

IX.

On the Compounds of Sulphur and Oxygen. By Thomas Thomson, *M. D, Lecturer on Chemiftry in Edinburgh. From the Author.*

Three known compounds of fulphur and oxygen, 1. oxide, 2. acids.

It is at prefent the opinion of Chemifts that fulphur is capable of combining with three dofes of oxygen, and of forming three diftinct compounds, namely,

1. Oxide of Sulphur.
2. Sulphurous Acid.
3. Sulphuric Acid.

The

The firſt of theſe is ſuppoſed to contain a minimum, the third a maximum of oxygen. Of theſe three the conſtituents of the laſt only have been aſcertained with preciſion. It will be proper to begin with it, as the knowledge of its compoſition may be of ſervice in aſcertaining the conſtituents of the reſt.

I. *Of Sulphuric Acid.*

This acid has been lately analyſed with preciſion by Thenard and Chenevix. I have repeated their experiments with care, and have obtained for the mean reſult 39 per cent. of oxygen, which is only one half per cent. greater than the reſult obtained by Mr. Chenevix. This difference in the preſent ſtate of analyſis may be accounted altogether inſignificant. I ſhall conſider ſulphuric acid, then, as compoſed of

Sulphuric acid, compoſed of 61 ſulphur and 39 oxygen.

61 ſulphur
39 oxygen
———
100

II. *Of Sulphurous Acid.*

Moſt of the properties of this Acid have been long known to chemiſts; but no experiments have been made to aſcertain the proportion of its component parts. Before I proceed to relate the reſult of mine, it may be worth while to deſcribe a few of the properties of Sulphurous Acid, which have not hitherto been ſtated with preciſion.

Sulphurous acid.

1. Fifty-three meaſures of ſulphurous acid gas were introduced into a graduated tube ſtanding over mercury, and one meaſure of water was thrown up. In five minutes 20 meaſures of gas were abſorbed, and in 24 hours the abſorption amounted to 33 meaſures. No farther abſorption took place in three days more; but on introducing the tube into water, the whole gas diſappeared, except a ſmall globule, which did not exceed 1-10th of a meaſure. During this experiment the thermometer at the time of obſervation deviated very little from 61°, and the barometer oſcillated from 29.55 to 29.77. Water then, at the temperature of 61°, abſorbs 33 times its bulk of this gas. Now, if with Lavoſier, we ſuppoſe a cubic inch of gas to weigh 0.63 grs. a cubic inch of water will abſorb 19.79 grains of ſulphurous acid, and 100 parts of water will abſorb 8.21 parts by weight.

Water at 60° abſorbs nearly one-eleventh part of its weight of ſulphurous gas.

2. A current

The impregnated water is intensely acid. Sp. gravity 1.0513. Slight heat disengages the acid.

2. A current of sulphurous acid gas was passed through a large quantity of water till the liquid refused to absorb any more. The taste of the water thus saturated, was intensely acid and sulphureous, and its odour excessively strong. The specific gravity at the temperature 68° was 1.0513; the heat of the hand was sufficient to occasion an extrication of gas. When moderately heated, it frothed violently, and exhaled the dense blue smoke which usually indicates the presence of sulphurous acid. When boiled down in a retort to half its bulk, it lost its smell, but still continued slightly acid. Hence it obviously contained sulphuric acid.

Analysis of sulphites. The solution of sulphurous acid contains a small portion of sulphuric acid.

3. In analysing the different sulphites, I have not found barytes answer so well as I was led to expect from the experiments of Fourcroy and Vauquelin; the solubility of sulphite of barytes in water is so considerable, that precision by means of it is scarcely to be looked for. But nitrate of lead yields with the alkaline and earthy sulphites a white insoluble powder of sulphite of lead, which may be dried in the temperature of 300° without decomposition, and is then composed of about

25 sulphurous acid.
75 yellow oxide of lead.

100

One hundred parts of the above liquid sulphurous acid yielded, with nitrate of lead, a precipitate indicating the presence of 6.15 parts of sulphurous acid. Another hundred parts, boiled down to one half in a retort, yielded, with muriate of barytes, a precipitate indicating the presence of 0.34 sulphuric acid. Therefore, 100 parts of my liquid sulphurous acid contained about

5.81 sulphurous acid,
0.34 sulphuric acid,

6.15.

So that the sulphuric acid amounts nearly to $\frac{1}{17}$ of the sulphurous. The presence of this acid is a proof of an affinity between sulphurous acid gas and sulphuric acid; for the gas was passed through an intermediate vessel before it reached the water.

This contamination seems to render the water less capable of absorbing sulphurous gas.

4. The proportion of acid combined with water in the liquid sulphurous acid was rather less than 7; yet, when water is plunged into a large column of gas, we have seen that it absorbs

abforbs rather more than eight parts of it by weight. Perhaps this difference was owing to the prefence of the fulphuric acid in the liquid. For water, flightly acidulated with fulphuric acid, abforbs a fmaller proportion of gas, than pure water.

5. After trying various experiments, in order to afcertain the conftituents of fulphurous acid, I found the following method moft to be depended on.

Analyfis of fulphurous acid.

Sulphite of potafh was obtained by Berthollet's method. It is a fine white falt, the properties of which have been very fully detailed by Fourcroy and Vauquelin, though they have neglected to analyfe it.

Sulphite of potafh lofes fome weight by a low heat: by ignition it lofes 22.3, and fulphuric acid with the alkali are left.

When this falt is expofed for a few minutes to a heat of 300°, it lofes 3.3 per cent. of its weight; and fuffers no additional lofs, though the heat be continued for an hour. When heated to rednefs in a platinum crucible, it decrepitates, becomes of an opake white, a blue flame iffues from below the lid, and, on taking off the cover at that inftant, the falt may be obferved of a glowing red heat in the middle. When this glow difappears, the falt will be found to have fuftained a lofs of 22.3. per cent. and it lofes no more, though melted, and kept half an hour in fufion. On evolving, it fplits into the fine thin tranfparent plates, which diftinguifh fulphate of potafh in the fame circumftances. When this refidue is diffolved in water, and treated with muriate of barytes, this precipitate of fulphate of barytes obtained, when dried and heated to rednefs, weighs 95.5, indicating the prefence of 22,92 fulphuric acid. Suppofing with M. Chenevix, that fulphate of barytes contains 24 per cent. of fulphuric acid: hence it follows that fulphate of potafh is compofed of

22.30	volatile matter
22.25	fulphuric acid
55.45	potafh
100.00	

When 100 grains of fulphate of potafh were expofed to the heat of a lamp in a retort with a very long beak, fitted to a mercurial air holder, they decrepitated and affumed the appearance of an opake white powder: 18 cubic inches of gas were extricated, and fulphur, with a little water, was volatilized into the beak of the retort; the gas was abforbed by water, and had the ufual fmell of fulphurous acid: the retort had

The volatile matter is fulphurous gas, with fome fulphur and a little water.

had loſt 15.2 grains of weight. The ſulphur being carefully collected, was found to weigh 5.1 grains. When burnt, it left 0.1 of reſiduum, which ſeemed to be ſulphurate of iron, for it gave a yellow colour to muriatic acid;* the water volatilized could not be weighed, but I eſtimate it at 2 grains. The experiment ſhews us what the volatile matter is which is drawn off when ſulphate of potaſh is heated to redneſs. It is compoſed of

15.2	ſulphurous acid
5.1	ſulphur
2.0	water
22.3	

The ſalt which remained in the retort being diſſolved, and treated with muriate of barytes, gave a precipitate which indicated the preſence of 23.2 of ſulphuric acid. Hence ſulphate of potaſh is compoſed of

Component parts of ſulphite of potaſh.

23.2	ſulphuric acid
15.2	ſulphurous acid
5.1	ſulphur
54.5	potaſh
2.0	water
100.0	

But it is obvious that, before the application of heat, the firſt three conſtituents together conſtituted ſulphurous acid. Hence ſulphite of potaſh is compoſed of

43.5	ſulphurous acid
54.5	potaſh
2.0	water
100 0	

Explanation of the action of heat on ſulphite of potaſh.

This analyſis enables us to trace the changes produced upon ſulphite of potaſh by heat. A temperature of 300° ſeparates the water and a ſmall portion of ſulphurous acid, which ſeems more looſely combined; for the ſalt, in conſequence, loſes its ſmell; an increaſe of heat occaſions a ſeparation of a portion of the acid, unaltered; the remainder divides itſelf into two parts, namely, ſulphuric acid, which remains combined with the potaſh, and ſulphur, which ſublimes. Hence

* I have never yet burned ſulphur, without obſerving traces of a ſimilar reſiduum.

we

we learn, that fulphurous acid is compofed of 23.2 fulphuric acid, and 5.1 fulphur, which gives us

82 fulphuric acid
18 fulphur
———
100

But 100 parts of fulphuric acid contain 39 of oxygen; therefore 82 contain nearly 32. Hence fulphurous acid is compofed of

68 fulphur
32 oxygen
———
100

Fourcroy affirms, that fulphurous acid contains only about 15 per cent. of oxygen, which is lefs than one half of the refult juft given. But he quotes no experiment in proof of his affertion. In all probability it was a mere guefs.

Sulphurous acid is moft probably fulphur combined with fulphuric acid.

6. The phenomena which attend the acidification of fulphur and the decompofition of fulphurous acid, render it probable that fulphurous acid is rather a compound of fulphuric acid and fulphur, than of fulphur and oxygen.

Sulphur and fulphuric acid combine with great facility. For if we form them into a probe, a very moderate heat is fufficient to convert the whole into fulphurous acid gas.

Whenever fulphur is acidified, a portion of fulphuric acid always makes its appearance in whatever way the procefs is conducted. Such at leaft has been the conftant refult of my experiments.

When fulphur is expofed to the heat of an Argand lamp in a retort connected with a mercurial air holder, it melts and fublimes at firft rapidly, but much more flowly, when the procefs has continued for fome time. In a retort, whofe capacity was 63 cubic inches, four hours elapfed before ½ oz. of fulphur was fublimed into its neck. A confiderable quantity of air was driven over; but on allowing the veffels to cool, the whole returned again, except 3 cubic inches. So that, by the operation, the air in the retort had increafed about $\frac{1}{20}$th part. It fmelt very pungently of fulphurous acid. When agitated in water, a fmall portion of it difappeared. The water did not acquire a perceptible tafte, but it precipitated muriate of barytes even after being boiled for fome time. A portion of this air, after being well wafhed, was left in contact with a ftick of phofphorus over water. Its bulk was di-

minifhed 17 per cent. Hence it had loft 5 per cent. of oxygen by the action of the hot fulphur on it. Here we fee the fource of the acidification of the fulphur during its fublimation. From this experiment we are authorized to conclude that both fulphuric and fulphurous acids may be formed merely by heating fulphur in common air, without any fenfible combuftion.

Sulphur, whenever fublimed, is in part acidified.

Sulphuric acid feems to be formed whenever fulphur is fublimed. For every fpecimen of flowers of fulphur which I have had an opportunity of examining, contained that acid. If common flowers of fulphur be boiled in water, the liquid always precipitates muriate of barytes. But flowers of fulphur, when once they have been well wafhed and dried, communicate no fuch property to water. If we now fublime thefe very flowers a fecond time, water in which they are boiled, precipitates muriate of barytes, as at firft.

Combuftion of fulphur under a jar over water.

When a red hot glafs capfule is rapidly placed on a pedeftal of ftanding water, fulphur thrown into it, and a glafs jar fuddenly put over it, the combuftion of the fulphur continues for a confiderable time; denfe bluifh-white fumes fill the jar, and at laft conceal the flame completely. The fmoke foon fubfides when the combuftion is over, and the water rifes flowly in the jar. By this procefs, the air in the jar lofes uniformly 8 per cent. of oxygen; it retains the fmell of fulphurous acid, even though allowed to remain over water for a week. But the fmell difappears in an inftant, if the air be paffed through water. A portion of the water over which the jar ftood, being treated with muriate of barytes, yielded a precipitate which weighed 8. An equal portion of the fame water evaporated to one-fourth, yielded a precipitate which weighed 7.

Action of acids on the fulphites.

7. The action of the more powerful acids upon the fulphites deferves attention, becaufe it ferves to illuftrate the nature of fulphurous acid. This action has been defcribed with confiderable minutenefs by Fourcroy and Vauquelin; but as the refult of my experiments differs a little from theirs, a few obfervations may not be unacceptable to the chemical reader. To prevent tedioufnefs, I fhall confine my remarks to fulphite of potafh.

Action of fulphuric acid upon fulphite of potafh.

When fulphite of potafh is thrown into concentrated fulphuric acid, a confiderable heat is evolved, a violent effervefcence takes place, and the falt lofes 48 per cent. of its weight.

weight. The heat of boiling water renews the effervefcence, and occafions a lofs of weight, amounting to 2 per cent. more. So that fulphite of potafh, when treated with fulphuric acid, lofes uniformly the half of its weight. Yet it contains only 43.5 per cent. of fulphurous acid. The additional 6.5 parts may be afcribed perhaps to the efcape of fulphuric acid along with the gas; for it can fcarcely be doubted that there is an affinity between them. When the fulphuric acid folution is fet afide, brilliant plates of fuper-fulphate of potafh foon make their appearance in it.

When fulphite of potafh is thrown into muriatic acid, a violent effervefcence enfues, but no increafe of temperature; and the falt lofes 34 per cent. of its weight. The heat of boiling water renews the effervefcence, and occafions a farther lofs of 16 per cent. making the whole lofs amount to 50, as in fulphuric acid. From this experiment we fee that muriatic acid does not expel the whole of fulphurous acid, unlefs affifted by heat: and in that cafe, a portion of the muriatic acid is driven off at the fame time with the fulphurous. When the muriatic acid folution is fet afide, beautiful arborefcent cryftals of muriate of potafh make their appearance in it. Muriatic acid and fulphite of potafh.

I diffolved 500 parts of fulphite of potafh in water, and putting the folution in a Woulfe's bottle, caufed a current of oxy muriatic acid gas to pafs through it; the gas paffed afterwards through a fecond bottle of water connected to the firft by a bent glafs tube. After the procefs the bottles were fet afide, till the green colour, occafioned by the oxy-muriatic acid, difappeared, and the fetid animal odour which ufually fucceeds that colour, was become perceptible. From the firft bottle I obtained, by means of muriate of barytes, a precipitate which weighed 777 parts, indicating the prefence of 37.3 per cent. of fulphuric acid. But as the fulphurous acid originally prefent amounted to 43.5 per cent. had it been wholly converted into fulphuric acid, not lefs than 48.5 per cent. of fulphuric acid would have been obtained, there was a lofs then of 11.2 per cent. of courfe, 10.5 parts of fulphurous acid muft have been diffipated by the action of the oxy muriatic acid. Accordingly the liquid in the fecond phial gave an abundant precipitate with muriate of barytes: and this precipitate, contrary to what I expected, confifted chiefly of Oxy muriatic gas and fulphite of potafh.

ſulphite of barytes; for the greater part of it was ſoluble in ſulphurous acid.

Nitric acid and ſulphite of potaſh.

When ſulphite of potaſh is thrown into concentrated nitric acid, a violent efferveſcence takes place, and much heat is evolved, the loſs of weight is 44.5; the liquid treated with nitrate of barytes, gives a precipitate, which indicates the preſence of 39.6 of ſulphuric acid. Hence we ſee that the loſs of weight during the efferveſcence was owing chiefly to the eſcape of nitrous gas.

When the aoid is diluted, the efferveſcence is violent, but no heat is evolved, and the ſmell of ſulphurous acid gas making its eſcape is very perceptible. The loſs of weight is only 12 per cent. the reſiduum, treated with nitrate of barytes, gave a precipitate indicating the preſence of 43.2 per cent. of ſulphuric acid. Here we ſee that moſt of the loſs of weight was owing to the eſcape of ſulphurous acid: yet the greater part was converted into ſulphuric acid.

Compoſition of ſulphite of potaſh.

8. During the courſe of theſe experiments I had occaſion to examine the compoſition of ſulphate of potaſh; and as my reſults differ a little from thoſe ſtated by others, it will be proper to notice ſome of them in this place.

When ſulphite of potaſh is heated to redneſs in a platinum crucible, the reſiduum poſſeſſes the properties of ſulphate of potaſh. It may be fuſed without any loſs of weight, and when diſſolved and cryſtallized again, we obtain the ſame ſalt as at firſt: 100 parts of this ſalt precipitated by muriate of barytes, yields a precipitate which, after being heated to redneſs, weighs, at a medium, 96 parts, indicating about 23 per cent. of ſulphuric acid. Hence this ſulphate is compoſed of

23	acid
67	potaſh
100	

When ſulphuric acid is ſuperſaturated by means of carbonate of potaſh, we obtain by evaporation the common ſulphate of potaſh of chemiſts: the ſame ſalt ſeparates in cryſtals during the purification of the potaſh of commerce. When the ſalt is reddened in a platinum crucible, it loſes 1.4 per cent. of its weight, and no more, though it be kept in fuſion. Diſſolved in water, and treated with muriate of barytes, it yields a precipitate which weighs 128.5; the mean of three experiments differing

differing from each other not more than 3.5 per cent.* Hence it contains 30.84 ſulphuric acid. This ſulphate, then, is compoſed of

30.84	acid
67.76	potaſh
1.40	water
100.00	

When ſulphite of potaſh is left for ſome months expoſed to the air, and then heated to redneſs, it yields with muriate of barytes a precipitate indicating about 38 per cent. of ſulphuric acid.

The ſuperſulphate of potaſh loſes 26 per cent. in a red heat, and the remaining 74 parts diſſolved in water, and treated with muriate of barytes, yield a precipitate indicating the preſence of 30.4 ſulphuric acid, whereas 100 parts of the ſuperſulphate diſſolved in water, without being previouſly heated, yield, with muriate of barytes, a precipitate indicating the preſence of 38.4 ſulphuric acid. Hence it follows that the ſalt is compoſed of

38.4	acid
43.6	alkali
18.0	water
100.0;	

or, abſtracting the water, of

46.4	acid
53.6	alkali
100.0	

III. *Oxide of Sulphur.*

Sulphur uſually occurs in one or other of three ſtates; namely, 1. A whitiſh powder, formerly diſtinguiſhed by the name of *lac ſulphuris*. 2. In rolls or flowers of a greeniſh yellow colour. This is the ſulphur of commerce. 3. In the ſtate of a reddiſh yellow, pitchy ſubſtance. This is commonly employed for forming the caſts of medals, &c. known by the name of ſulphurs.

External characters of ſulphur.

* Mr. Vauquelin affirms, in his Diſſertation on the Potaſh of Commerce, that this ſalt yields with muriate of barytes a precipitate amounting to 22°, the weight of the ſalt. My experiment differs very much from this ſtatement.

1. It

Lac sulphuris, or white sulphur, is sulphur and water; yellow sulphur is the pure.

1. It is well known that sulphur, when first obtained by precipitation from any liquid, is always of a white colour, which gradually changes to greenish yellow when the sulphur is exposed to the open air. If this white powder, or *lac sulphuris*, as it is called, be exposed to a low heat in a retort, it soon acquires the colour of common sulphur; and, at the same time, a quantity of water is deposited in the beak of the retort. On the other hand, when a little water is dropt into melted sulphur, the portion in contact with the water immediately assumes the white colour of *lac sulphuris*. If common sulphur be sublimed into a vessel filled with the vapour of water, we obtain *lac sulphuris* of the usual whiteness, instead of the usual flowers of sulphur. These facts prove that *lac sulphuris* is a compound of sulphur and water. Hence we may conclude that greenish yellow is the natural colour of sulphur. Whiteness indicates the presence of water.

Sulphur rendered viscid and dark-coloured by fusion.

2. It has been long known, that when a considerable quantity of sulphur is kept melted for some time in an open vessel, it becomes viscid, changes its colour to a dark violet, and acquires a kind of pitchy appearance. The nature of this change has not hitherto been examined by chemists. Fourcroy, indeed, affirms, that the sulphur, in this case, is in the state of an oxide. But the assertion does not seem to have been the result of any positive experiment.

Does not succeed in a shallow vessel.

I have never been able to produce this change in the appearance of sulphur by heating it in a flat dish, where nothing impedes the volatilization, though I have kept it melted in a glass capsule on sand, heated to 250°, for ten hours together. But the change takes place in a short time, when a considerable quantity of sulphur is kept melted in a crucible; and the greater the quantity employed, the sooner the change is produced, and the more complete it is.

This supposed oxide is of a violet colour; is soft if poured into water; one-sixth denser than sulphur; and tough.

When sulphur, thus converted into a supposed oxide, is newly prepared, its colour is a dark violet, with the metallic lustre; not very unlike newly-melted muriate of silver, when seen by reflected light. If it be thrown suddenly, while in fusion, into water, it continues soft for a considerable time; and, as it hardens, the colour changes from purple to reddish yellow. When broken, it exhibits a fibrous fracture, composed of small prismatic crystals. Its specific gravity was 2.325. It was very tough, resisting, with a good deal of ob-

 stinacy,

ftinacy, the action of the peftle. The powder had a ftraw yellow colour. Its properties differ, we fee, from roll fulphur, which is remarkably brittle, and whofe fpecific gravity does not exceed 2.

To afcertain whether this fuppofed oxide really contained oxygen, I treated 100 parts of it with nitric acid till the whole was converted into fulphuric acid. The procefs was as tedious as the acidification of common fulphur, by means of the fame acid. By nitrate of barytes I obferved a precipitate, which, after being reddened in a platinum crucible, weighed 667, indicating 160 parts of fulphuric acid; the fuppofed oxide had abforbed, of courfe, 60 parts of oxygen. Hence we have fulphuric acid compofed of

It feems to contain a fmall portion of oxygen, though perhaps not enough to entitle it to the name of oxide.

62.5	fuppofed oxide
37.5	oxygen
100.0	

But 100 part of pure fulphur would have abforbed nearly 64 of oxygen, and formed 164 of fulphuric acid. Hence it follows that the fuppofed oxide is compofed of

97.6	fulphur
2.4	oxygen
100.0	

Though the refult of a fimilar experiment was nearly the fame; yet the proportion of oxygen is certainly too fmall to authorize us, in the prefent ftate of chemical analyfis, to conclude that the fuppofed oxide really contains $2\frac{1}{2}$ per cent. of oxygen: for fo fmall a deviation from the compofition of fulphuric acid, by acidifying common fulphur, as $2\frac{1}{2}$ per cent. may, very probably, be owing to an error of analyfis. At the fame time the uniformity of my refults inclines me to believe that this fuppofed oxide of the French chemifts really contains fome oxygen.

3. As no fatisfactory refult was likely to be obtained by expofing fulphur to heat and air, it became neceffary to try the effects of thofe chemical agents which are capable of communicating oxygen to other bodies. Sulphuric acid could not be ufed, becaufe fulphur converts it into fulphurous acid; the effect of nitric acid was well known; but the action of oxymuriatic acid had not been tried. Some of the foreign chemifts, indeed,

Oxigenation of fulphur by compound agents.

indeed, affirm that ſulphur takes fire when plunged into that gas; but they muſt have, ſome how or other, deceived themſelves.

Oxigenation by oxymuriatic acid. Sulphur expoſed to ox. mur. acid gas was converted into the *new product of a fine red liquid.*

I connected three Woulfe's bottles in the ſame manner, by means of glaſs tubes; furniſhed each with Welter's tubes of ſafety. The firſt contained an ounce troy of pure dry flowers of ſulphur; the ſecond was filled two-thirds with diſtilled water; and the third with a weak ſolution of cryſtallized carbonate of potaſh. A current of oxymuriatic acid gas was made to paſs through theſe bottles in the uſual way. The proceſs laſted a conſiderable time. The firſt bottle was ſoon filled with the greeniſh fumes of the gas; the ſulphur gradually became moiſt and doughy, and the particles of it which adhered to the ſides began to trickle down in drops; its colour changed to orange, and at laſt, a fine red liquid made its appearance. The whole of the ſulphur was gradually converted into this liquid. I then ſtopt the proceſs. Abundance of gas had paſſed through all the bottles: the water in the ſecond was, at one time, quite milky, but it recovered its tranſparency before the proceſs was finiſhed. Dots of ſulphur were depoſited along the glaſs tubes which connected the firſt and ſecond phials; but none in that which connected the ſecond and third, the ſolution in the third phial effervеſced preciſely as in the uſual proceſs for preparing hyper-oxymuriate of potaſh. The gas which eſcaped was carbonic acid. No oxymuriatic acid gas could be diſtinguiſhed by its ſmell in the ſmall capſule of water into which a tube iſſuing from the third bottle was plunged.

— denominated *ſulphureted muriatic acid.*

As the red liquid obtained by this proceſs has never before been examined by chemiſts, and, as it differs very much in its properties from all other ſubſtances at preſent known, it will be neceſſary to diſtinguiſh it by a peculiar name, I ſhall call it *ſulphureted muriatic acid,* till ſome better name be thought of.

— more than double the weight of the ſulphur.

This liquid amounted to 1¾ ounce meaſures, excluſive of what adhered to the ſides of the bottle; its ſpecific gravity was 1.623. It amounted, therefore, to 2.63 ounces, or more than twice the weight of the ſulphur, excluſive of what had been volatilized during the proceſs.

It is green by tranſmitted light.

Sulphureted muriatic acid is perfectly liquid; its colour is a fine red, intermediate between ſcarlet and crimſon. When ſtreaks of it run down the inſide of the phial, they appear green by tranſmitted light.

When

When expofed to air, it fmokes at firft almoft as intenfely as the fmoking oxymuriate of tin of Lebavius; but the intenfity gradually diminifhes, and, at laft, refembles that of the moft concentrated muriatic acid a little heated. It is very volatile, difappearing very rapidly when expofed to a moderate heat.

It fmokes, and is very volatile; tafte acid; reddens blue colours; gives white fumes by approach of ammonia; and when dropped into water, leaves a film of fulphur, and gives yellow acid, ductile, infoluble flakes that continue foft in the air.

Its fmell has a ftrong refemblance to that of fea-plants, but is much ftronger. The eyes, when expofed to its fumes, are foon filled with tears, and acquire the fame painful feeling as when expofed to the fmoke of wood or peat.

Its tafte is ftrongly acid, hot, and bitter, affecting the throat with a painful tickling.

It converts vegetable blue papers to red; but the change takes place flowly, unlefs the paper be dipt into water; the paper is not corroded unlefs heat be applied.

When brought near a phial of ammonia, denfe white fumes of muriate of ammonia make their appearance. If it be held above a folution of nitrate of filver, yellow flakes precipitate in abundance.

If a drop of fulphureted muriatic acid be let fall into a glafs of water, the furface of the water becomes immediately covered with a film of fulphur; a greenifh red globule falls to the bottom, which remains for fome time like a drop of oil, but at laft is converted into yellow flakes; thefe flakes have an acid tafte, which they do not lofe, though allowed to remain in water for feveral days; they are very ductile, and continue fo, though left expofed to the air.

Chemical examination of the red liquid or *fulphureted muriatic acid.*

4. To afcertain the conftituents of this liquid I agitated 110 parts of it in a very weak folution of potafh, and then threw the whole on a filter: a yellow ductile fubftance was feparated, which adhered very ftrongly to the filter; what I could feparate was put on a plate of glafs, and dried gently in the open air. It weighed 40. This fubftance had a yellow colour, and refembled half dry oil paint; its tafte was hot; it adhered fo obftinately to the finger, that feveral days elapfed before every trace of it difappeared. When digefted for fome time in hot water, it fell into flakes of fulphur, and the water acquired an acid tafte. The flakes weighed only 34, and poffeffed all the properties of common fulphur. The water in which it had been digefted yielded, with nitrate of barytes, a precipitate which weighed 8, indicating the prefence of 1.92 fulphuric acid with nitrate of filver; the precipitate amounted to 16, indicating

Chemical examination of the red liquid or *fulphurated muriatic acid.*

ing 2.88 muriatic acid: but the precipitate had a brownifh tinge, of which muriate of filver is deftitute. The 40 parts of yellow refiduum then contained

34.00	fulphur
1.92	fulphuric acid
2.88	muriatic acid
1.20	water or lofs
40.00	

Seventy parts ftill remain to be afcertained. They muft exift in the folution of potafh. This folution, fuperfaturated with nitric acid, and treated firft with nitrate of barytes, and next with nitrate of filver, yielded precipitates indicating the prefence of 4.8 fulphuric acid and 36.45 of muriatic acid. The refiduum yield no farther precipitate with filver; but, when evaporated to drynefs, fome yellow cryftals were obtained, which gave traces of fulphur, but in too fmall quantity to be weighed. This analyfis gives us the following proportions:

	35.00	fulphur
	39.33	muriatic acid
	6.72	fulphuric acid
	81.05	
	28.95	lofs
	110.00	
or per cent.	31.82	fulphur
	35.75	muriatic acid
	6.10	fulphuric acid
	73.67	
	26.33	lofs

This enormous lofs was owing, at leaft, in part, to the improbability of feparating from the filter, the whole fulphureous mafs fo as to weigh it. This induced me to have recourfe to the following method of afcertaining the proportions of fulphur in fulphureted muriatic acid.

Remarks on the fulphuret of muriatic acid.

When fulphureted muriatic acid is thrown into warm nitric acid a very violent effervefcence takes place, and the whole mixture is thrown, with a kind of explofion, out of the veffel. If the acid be cold, the effervefcence is at firft flow, but heat is very foon evolved, and the fame effects produced. When the proportion of nitric acid is great, and the fulphuret dropt in

in very flowly, the effervefcence continues moderate; nitrous gas and oxymuriatic gas being evolved, as was evident, from the fmell. I diffolved 100 parts of the fulphuret in nitric acid. The liquid yielded, with nitrate of barytes, a folution which, when properly dried, weighed 282 parts, indicating 67.6 parts of fulphuric acid; but 67.6 of fulphuric acid contain 26.3 oxygen. Hence 100 parts of fulphureted muriatic acid contain 41.3 of fulphur.

Chemical examination of the red liquid or *fulphureted muriatic acid.*

If a cryftal of muriate of barytes be fufpended in fulphureted muriatic acid, no precipitate takes place; neither was any obtained by agitating this falt in powder with the fulphuret. Hence I conclude, that the fulphuret does not contain fulphuric acid; but that fulphuric acid is formed whenever the liquid comes in contact with water. The oxygen cannot be fuppofed to have been previoufly united to the muriatic acid folution. For I find, by trial, that if the oxymuriatic acid gas be ftill made to pafs through the fulphuret, after it is once formed, fulphuric acid immediately makes its appearance in it. The oxygen then muft have been combined with the fulphur; the whole of which was in the ftate of an oxide. Whenever the fulphuret is diluted with water, that oxide undergoes decompofition, one portion of it abftracting the whole of the oxygen from the other, fo that fulphuric acid and fulphur make their appearance together.

If we fuppofe the proportion of fulphur and fulphuric acid obtained by the firft analyfis to be that which is formed when the fulphur is mixed with water, we fhall have the oxide of fulphur compofed of

	31.82	fulphur
	6.10	fulphuric acid
or of	35.54	fulphur
	2.38	oxygen
or per cent. of	93. 8	fulphur
	6. 2	oxygen
	100. 0	

But the fulphuret yielded to nitric acid 41.3 per cent. of fulphur. Hence it muft contain 44 per cent. of oxide of fulphur. Sulphureted muriatic acid then contains

44.00	oxide of fulphur
35.75	muriatic acid
20.25	lofs
100.00	

Chemical examination of the red liquid or *fulphureted muriatic acid.*

The lofs is ftill very confiderable. Probably the greater part of it is owing to the prefence of water; the amount of which it is impoffible to afcertain.

The above analyfis conveys but an inadequate idea of the conftitution of fulphureted muriatic acid, becaufe the proportions of its conftituents vary confiderably, according to the procefs. The longer the procefs is continued the greater is the encreafe of muriatic acid, and the fmaller the proportion of oxide of fulphur. I found a portion of fulphuret thus formed to contain

47.1	muriatic acid
35.2	oxide of fulphur
4.0	fulphuric acid
86.3	
13.7	lofs

It was this laft fulphuret that was obtained in the procefs in which the quantity of fulphuret which I got, as ftated above, was meafured, the fecond Woulfe's bottle contained a folution of muriatic acid and fulphuric acid in water. Hence we fee that the fulphuret, after being formed, had been partly covered over by the oxymuriatic acid gas. The fulphuric acid obtained, by means of barytes, amounted to 36 grains; the muriatic acid to 139 grains. The third vial contained no fulphuric acid, but confifted of a mixture of muriate of potafh, hyperoxymuriate of potafh and carbonate of potafh.

Remarks on the fulphureted muriatic acid.

5. The fulphuret of muriatic acid claims the peculiar attention of chemifts, not only on account of its compofition, which our previous knowledge would have induced us to confider as impoffible, but on account of the many remarkable properties whch it difplays. As I mean to referve a full account of its properties for a fubfequent paper, I fhall fatisfy myfelf at prefent with the following remarks.

Sulphuret of muriatic acid diffolves phofphorus.

1. Sulphuret of muriatic acid diffolves phofphorus cold with great facility. No effervefcence takes place; the folution has a fine amber colour, and is permanent. When evaporated, the phofphorus remains behind with a little fulphur, and at laft takes fire. When the folution is mixed with liquid potafh, the whole becomes beautifully luminous, and phofphuret of fulphur is precipitated.

Sulphuret effervefces with alcohol, and forms ether.

2. When mixed with alcohol, a violent effervefcence is produced, ether is immediately difengaged, and, what I did not

not expect : this ether is mixed with sulphurous acid, and must be rectified in the same way as sulphuric ether, which it resembles in smell.

3. All the acids decompose this sulphuret, sulphur usually precipitating, except liquid sulphurous acid, which produces no change, and nitrous acid, which dissolves and decomposes it at the same time. Is discomposed by all acids;

4. The fixed alkalies dry produce with it a violent effervescence and a very high degree of heat. When ammoniacal gas is passed through it, the vessel is filled with a fine purple sal-ammoniacal smoke, the whole becomes solid and of a deep red colour; but when mixed with water, sulphur is immediately precipitated. habitudes with alkalis;

5. This liquid precipitates silver of a yellow colour mixed with white, the white is a muriate of silver, the yellow is a compound of the oxides of silver and sulphur. It becomes brown when dry. Nitric acid decomposes it, dissolving the silver and acidifying the sulphur. precipitates silver yellow and white.

IX.

Further Experiments and Observations on the Efflorescences of Walls. In a Letter from Dr. BOSTOCK.

To Mr. NICHOLSON.

SIR,

SINCE I sent you the analysis of the saline efflorescence from the walls of Mr. Earle's house, inserted in your journal for November last, I have had an opportunity of examining four other efflorescences obtained from brick walls, the particulars of which I shall now detail. Four efflorescences from different walls.

The first of these was presented to me by my friend Dr. Rutter, who discovered it in considerable quantity on the top of the walls of his house just below the roof. This salt had in every respect the same external characters with the one which I had before examined, and upon submitting it to the action of the same chemical re-agents, similar results were obtained. In addition to the former experiments, I compared the effects produced upon it by pure ammóniac, and by the carbonate of ammoniac; the former threw down a copious preci- The first proved to be pure sulphate of magnesia.

precipitate, while the latter had no perceptible operation; a decisive test of the existence of magnesia, which was suggested to me by Mr. William Henry of Manchester. This salt appeared, therefore, to be a very pure sulphate of magnesia.

The second efflorescence was sulphate of soda with indication of muriatic acid.

The second efflorescence which I examined was obtained from the outer wall of a stable, which had been erected for some years. It differed on its appearance from the two former; instead of shooting out from the wall in spiculæ of considerable length, it appeared like a powder scattered over the surface, occupying distinct, round patches, so as in some degree to resemble the growth of a grey lichen. The bricks on which this efflorescence appeared were in general of a softer texture than those of the rest of the wall. Though it occupied a considerable extent, it was difficult to collect it in any quantity, but I obtained sufficient to subject it to the following experiments. The salt was dissolved in warm water, filtered and crystallized; the crystals were very soluble at the common temperature of the atmosphere. Muriate of Barytes added to the solution produced a copious precipitate; carbonate of pot-ash, pure pot-ash, pure ammoniac and oxalic acid were respectively added to the solution, but produced no effect. Nitrate of silver caused a precipitate, but only in small quantity. From these experiments it appeared that the salt in question consisted of the sulphuric acid, mixed with a small proportion of the muriatic, and combined with one of the fixed alcalies. From the form of its crystals I conceived that the sulphuric acid was in this case united to soda, but the quantity of salt which had been procured, was not sufficient to enable me to determine accurately from this circumstance. A more decisive test between the sulphate of soda and the sulphate of pot-ash, is the property which the latter alone possesses of forming alum with the acid sulphate of alumine. I accordingly prepared a quantity of this substance; to one portion of it sulphate of pot-ash was added, and to the other some of the salt under examination. By gentle evaporation and subsequent cooling, the first produced very evident crystals of alum, the latter only formed a confused mass. This I considered as a sufficient proof that this saline efflorescence was the sulphate of soda.

Third efflorescence sulphate

The third efflorescence was obtained from the inner walls of a brick house which was then erecting; it had all the internal cha-

characters of the ſaline efflorescence from the ſtable, and when ſubmitted to the ſame chemical re-agents, differed from it only in exhibiting ſlighter traces of the muriatic acid; this ſalt was therefore a ſulphate of ſoda nearly in a ſtate of purity.

of ſoda nearly pure.

The walls of the ſalt water baths in this town are covered with a ſtucco, which is in ſeveral places bliſtered and mouldering away. The parts of the plaiſter which are decaying, are covered with a copious efflorescence, which has the appearance of a fine white down. Some of this I collected and examined. It was diſſolved in warm water and filtered; the ſolution was not capable of being regularly cryſtallized, but formed a white maſs, eaſily ſoluble, poſſeſſing the acrid taſte of a fixed alcali, and affecting the colour of teſt papers in the ſame manner. A briſk effervescence was excited by the addition of an acid, and from this circumſtance and the effect of the different re-agents, I conceived it to be one of the fixed alcalies. In order to determine whether it was an uncombined alcali, and to which of theſe bodies it ought to be referred, I aſcertained what quantity of the ſulphuric acid was requiſite to ſaturate a known weight of the ſalt, and afterwards, employing the ſame acid, compared it with the quantity which the ſame weight of alcali required. As the ſalt had not attracted any moiſture from the atmoſphere during a period of ſome weeks, I concluded it to be ſoda, and I accordingly found that the ſame quantity of ſulphuric acid ſaturated equal weights of ſoda, and of the ſalt under examination; the ſolution being ſlowly evaporated, formed well-marked cryſtals of the ſulphate of ſoda. That part of the walls of the bath on which the ſalt had effloreſced in the greateſt quantity, was out of the reach of the immediate action of the ſea water; but it is probable that the ſand of the ſhore had been mixed with the lime, for by examining a quantity of water which had been digeſted upon a portion of the plaiſter, it yielded a very copious precipitation by the nitrate of ſilver, and this rendered it highly probably that the mortar contained the muriate of ſoda. It might therefore be conjectured that the ſoda in this caſe was accidentally formed by the ſame proceſs which, according to Mr. Accum's account in the 2d vol. of the Journal, p. 243, is employed deſignedly in Pruſſia for obtaining it, by the decompoſition of common ſalt.

Fourth effloreſcence, on the ſtucco of a ſea-water bath, was carbonate of ſoda,

Probably formed by proceſs formerly deſcribed from ſea water.

I ſhall

Investigation of the origin of the sulphate of magnesia on the bricks of Mr. Earle's house.

I shall conclude this communication by detailing to you the progress which I have made in investigating the origin of the sulphate of magnesia, which appeared in Mr. Earle's house. Before proceeding farther, it will be proper to observe that the efflorescence was here altogether confined to the bricks, the mortar which united them being entirely free from it, and that rain water only had been employed in tempering the clay. It remained therefore to examine with accuracy, whether any salt, soluble in water, existed ready formed in the clay, and what were the component parts of the clay itself. To ascertain the first of these points, 60 grains of the clay powdered and dried were well washed with boiling water; the water was filtered and evaporated, and the residuum carefully collected; it did not weigh $\frac{1}{4}$ of a grain. It was re-dissolved in water; it produced a copious precipitate with the muriate of barytes, and the nitrate of silver; a very faint cloud with oxalic acid and with pure pot-ash; ammoniac produced no effect. The ready formed salts appeared therefore to be the sulphate and muriate of a fixed alcali, with a minute portion of the muriate of lime, the whole however existing in very small quantity.

The clay contained a portion of muriate and sulphate of alkali,

And a larger portion of magnesia.

The clay itself was next examined; it was found to consist principally of silex and alumine in the proportion of about three to one; the quantity of lime was very small, though its existence was detected by the oxalic acid; its colour proved that it contained iron, and I also found that about five parts in the 100 consisted of magnesia. From this examination it appeared that one of the component parts of the salt exists in the clay; I attributed the formation of the sulphuric acid to the sulphur which is frequently met with in our coals, and which I conceived might unite with oxigen during the burning of the bricks. I attempted to put this action to the test of experiment, and accordingly I formed a paste of pipe clay and calcined magnesia in the proportion of 95 to five; this was placed in a crucible, surrounded with small coal mixed with a quantity of sulphur; the crucible was then kept for some time in a strong heat. I was not able to detect the presence of the sulphuric acid in the clay that was thus baked, but so many circumstances might actually take place in the formation and burning of the bricks, which we have it not in our power to imitate in the laboratory, that I do not consider the hypothesis disproved by my want of success.

Experiment to shew whether sulphur from the coals had given the acid? Doubtful.

In

In analyfing the clay I proceeded nearly upon the plan pointed out by M. Vauquelin in the 30th vol. of the Ann. de Chimie. Analyfis of the clay, &c.

This operation is however fo tedious, and requires fo much nicety in the management, that I made fome attempts to afcertain the exiftence of magnefia in the clay by a fhorter procefs. The firft method which I employed was fuggefted by an obfervation of Mr. Kirwan; he ftates that alumine is fufficiently difcriminated from magnefia by the greater folubility of the latter in dilute fulphuric acid; but it appears that the difference of folubility of thefe two fubftances cannot be employed as a teft of the prefence of magnefia where it exifts only in fmall proportion; for I found that the fulphuric acid diluted with above 200 times its weight of water, after being in contact with pure alumine for the fpace of 10 minutes only, had acted upon the alumine fo far that a precipitate was formed in the fluid by the addition of ammoniac.

The acetous acid is ftated as poffeffing a much more powerful action over magnefia than over alumine; but upon trial the fame objection occurred againft its ufe as in the former inftance.

The property which the magnefian falts poffefs of being decompofed by pure ammoniac but not by the carbonate of ammoniac, feemed to offer a method by which the fulphates of magnefia and of alumine might be feparated when mixed together in folution, and by which means confequently the prefence of magnefia might be detected in the clay under examination. But I found that though the fulphate of magnefia alone is not decompofed by the carbonate of ammoniac, yet that when a mixed folution of alum and the fulphate of magnefia is fubjected to the action of the carbonate of ammoniac, both the alumine and the magnefia are precipitated, fo that when the fluid is feparated by filtration, the addition of pure ammoniac produces no farther effect:

After I had made the unfuccefsful experiment related above refpecting the formation of the fulphuric acid, I received the laft number of your Journal, containing a communication from Mr. Gregor, in which he gives an account of the production of the fulphate of magnefia from the afhes of pit-coal. He attributes the production of this falt to the decompofition of the fchiftus and pyrites which are commonly found in coal, and Mr. Gregor's experiment not applicable to the cafe in queftion, as coal afhes were not ufed.

and by heating a mixture of thefe fubftances he fucceeded in forming the falt artificially. The fuccefs of his experiment feems to prove the truth of the theory, at leaft in the particular inftance in which he obferved the efflorefcence: but in thofe cafes where the falt evidently proceeds from the fubftance of the brick, and where the magnefia has been found to exift previoufly in the clay, the idea of Mr. Gregor appears lefs applicable, fo far as regards the origin of the magnefia; but in both the proceffes the fulphuric acid is equally fuppofed to be derived from the pyrites. Mr. Gregor fuggefts that coal afhes might have been mixed with the clay of which thefe bricks were formed; but I find upon enquiry that this was not the cafe.

I am,

Liverpool, Aug. 30, 1803.

Your obedient fervant,

JOHN BOSTOCK.

X.

Philofophical Obfervations on the Caufes of the Imperfection of evaporating Furnaces, and on a New Method of conftructing them, for the economical Combuftion of every Defcription of Fuel. By C. CURAUDAU, *correfponding Member of the Pharmaceutic Society of Paris* *.

On the combuftion of fuel and conftruction of furnaces.

NOTWITHSTANDING the attempts already made to introduce œconomy in the ufe of the combuftibles neceffary to the manufactures, we ftill ufe them with confiderable wafte. In all cafes a much greater quantity of fuel is confumed than is needful to keep up the ebullition in evaporating furnaces, or to produce the requifite temperature in furnaces for other purpofes. It may be eafily conceived that this fuperfluous confumption muft in large eftablifhments be attended with great lofs, and muft eventually tend to produce a fcarcity of fuel in the market. It therefore becomes us for both reafons to endeavour to prevent a fcarcity, of which future generations might with juftice accufe us of being the authors, unlefs we ferioufly occupy ourfelves in fearch of the methods of burning wood with more economy. Many very remarkable improvements

* From the *Annales de Chimie*, No. 138.

ments have indeed been made in the conftruction of fürnaces within thefe few years, but they are only advances towards perfection, and are yet very far from being carried to the extent of which they are capable. This will no doubt be the cafe with the alterations I am about to propofe; for thefe will enable us to make new obfervations, which moft probably will lead to further alterations ftill more important.

On the combuftion of fuel and conftruction of furnaces.

Concerning Evaporatory Furnaces.

The phyfical impoffibility of raifing the temperature in evaporating furnaces as they are at prefent conftructed, is one of the caufes which has always appeared to me moft ftrongly inimical to their improvement. For it muft not be imagined that the intenfity of the heat will be in proportion to the quantity of matter in ignition, or that caloric will not be more copioufly produced by the fame quantity of combuftible under certain circumftances than others; as for example, when the temperature is already very high, the products of heat from a combuftible fo fituated will be much more confiderable than thofe from the fame combuftible burned in a furnace, the temperature of which is conftantly depreffed by the evaporation of the liquid in the boiler.

To prove that it is only by virtue of a temperature already elevated that we can obtain an advantageous combuftion, I fhall take Argand's lamp, which will afford a comparative inftance on a fmall fcale of the effect produced by the intenfity of heat during combuftion. When thefe lamps have their glafs chimney, they afford a very brilliant light, and the oil will emit no fmoke. But if the chimney be taken off, the oil will immediately burn duller, the light will be lefs intenfe, and the wick will give out fmoke. This effect fhews that it is the current of air in the chimney, and the heat it keeps up round the wick, which contributes to the effect of the combuftion. What ftill adds weight to this opinion is, that the perfection to which this kind of lamp is brought depends principally on the form and proportions of its glafs chimney.

This example muft then naturally lead us to think that evaporating furnaces, as they are made at prefent, cannot advantageoufly promote combuftion, fince the bottom of the boiler, which is continually kept at the fame degree of heat by the evaporation of the liquid in boiling, conftantly prevents the

 rife

On the combustion of fuel and construction of furnaces.

rise of the temperature, whence it results that the heat which is insufficient to produce a complete combustion of the inflammable particles, will rather produce gazification than oxigination. This volatilization of the particles of combustible bodies which escape combustion, and which pass successively into the state of permanent gases, will also absorb a quantity of heat necessary to their gaseous constitution, which, together with the effects of the current of air, will tend to lower the interior temperature of the furnace, and to impede the process of combustion.

These remarks, which perfectly agree with all the phenomenæ of combustion, shew that the oxigen of the atmosphere does not act with much efficacy on combustible bodies, except when they are immersed in it at an high temperature, and that to apply an intense and uniform heat to an evaporating furnace without loss of the combustible, it should be produced in a fire-place, having a current of air, and so far distant from the boiler that the temperature may be raised gradually and at pleasure. By this means all the particles of the combustible matter will be in a state favourable to their oxigination; and the whole quantity of radiant heat produced by the reaction of the oxigen upon the combustible, will be disengaged and employed without loss.

That which under similar circumstances conduces still farther to encrease the action of the oxigen, is its continual renewal. For the higher the temporature of a furnace is raised the more easily the outer air will enter; and so likewise when the ignition is carried to a high degree, it becomes necessary and advantageous to check the current of air, not by closing the door or lower opening of the furnace, as is generally done, but rather by contracting or even closing the upper aperture of the chimney. By this means the heat becomes concentrated in the body of the furnace, and has no other passage than through the liquid in the boiler.

This remark on the method of checking the current of air by the top of the chimney, may also be applied to furnaces of fusion, and in cases where it is required to maintain the heat of a metal without exposing it to the oxigenating action of a current of air in a state of ignition.

General

General Remarks on the Construction of Furnaces.

That part of the fire-place which is to support the greatest heat, should be made of very refractory bricks. The best cement or mortar for bricks, in all cases where a bad conductor of heat is required, is a mixture of equal parts by measure of tan and clay. The tan prevents the cement from cracking, and produces an adhesiveness which, when dry, gives it a great degree of firmness.

Furnaces may also be constructed with this mortar, and on the same principles with those of evaporation, which I am about to describe.

Furnaces intended for strong heat, should be externally covered with a thick wall, constructed with the mortar of tan. By this means very little heat will be lost. All furnaces should be so constructed as to have the power of closing the upper aperture of the chimney at pleasure, in order to check the combustion, and concentrate the heat within the furnace, whenever this becomes necessary. When the temperature is very high, it is particularly necessary to regulate the issue of the current of air so as to prevent its too speedy circulation through the furnace, which, in certain cases, is prejudicial to the success of the operation.

By uniting all these conditions in furnaces, a certain saving of one fourth of the fuel will be made, and the combustion will be produced without any appearance of smoke. I insist more particularly on this remark, because it is clearly and physically proved that no combustible can be completely burned if smoke be produced.

Description of an Evaporating Furnace, in which the Temperature may be raised at Pleasure.

For common furnaces, the aperture of the vault A, Plate VI. should be four decimetres ($15\frac{3}{4}$ inches) wide, by three decimetres and a half ($13\frac{3}{4}$ inches) in height; B is the part of the vault in which the combustion is performed. This vault must be at least two metres ($6\frac{1}{2}$ feet) in length. C represents a boiler of one metre and a half (nearly five feet) in diameter, and of the same depth; it is set in a brick furnace. **Description of a furnace for evaporation.**

The interval from the bottom of the boiler to the base of the furnace must be at most one decimetre (about four inches.) It must be observed in the construction of furnaces, that the brickwork should be gradually sloped towards the boiler, and to reduce

duce the ſpace to about three centimetres (about an inch.) It muſt be thus continued to within one decimetre of the edge of the boiler; and muſt then be brought into contact with it. D is an aperture of two decimetres wide by one in height (about eight inches by four) communicating with E. But at the ſide of the angle *a*, this paſſage for the heat muſt be made of one metre (three feet three inches) in width, by one decimetre (about four inches) in height, and this proportion continued to the aperture E.

F is a ſecond boiler, intended to be heated by means of the exceſs of heat from the firſt; many others may be applied in ſucceſſion, if required. G is an aperture with the ſame proportions as D. At the angle *b*, it muſt be obſerved to make the aperture of the chimney five decimetres by two ($19\frac{1}{2}$ inch. by $7\frac{3}{4}$ inch.) and to continue this proportion to about two thirds of its height. The aperture may then be contracted ſo that, at its upper extremity, it may not be leſs than one decimetre by three (about four inches by twelve.) This part of the chimney ſhould be ſo conſtructed as to be able to cloſe it conveniently, when required.

XI.

Correction of a Miſtake in Dr. Kirwan's Eſſay on the State of Vapour in the Atmoſphere. By Mr. DALTON.

To Mr. NICHOLSON.

SIR,

Miſtake of Dr. Kirwan in quoting Mr. Croſthwaite as authority for a maximum height of the clouds.

I TAKE the liberty of requeſting you to correct a miſtake in Dr. Kirwan's eſſay on vapour, copied in your laſt number, page 216. In treating upon the height of clouds, he obſerves, "In lat. 54°, in Cumberland, Mr. Croſthwaite obſerved none "lower than 2700 feet, and none higher than 3150, in the "courſe of ſeveral years." For this he refers to my meteorological obſervations, page 41. The facts there ſtated, however, are ſo very different from thoſe above mentioned, that I conclude Dr. Kirwan has not ſeen the book, and has been miſinformed. The account referred to is, that Mr. Croſthwaite obſerved the heights of the clouds uſually three times a day for five years, by remarking their interſection with Skiddaw (a high mountain near Keſwick.) The reſult was,

Clouds from 0 to	100 yards above Derwent Lake,	10 times.
100 -	200	42
200 -	300	62
300 -	400	179
400 -	500	374
500 -	600	486
600 -	700	416
700 -	800	367
800 -	900	410
900 -	1000	518
1000 -	1050	419
above	1050	2098
	Total	5381

His obfervations could not be particular above 1050 yards, that being the perpendicular height of Skiddaw. There is not therefore any *maximum* of height fo much as hinted at; and the *minimum* is 0, or when the clouds reft on the ground, an event occurring in every part of Great Britain two or three times a year. Were we to form a conjecture from the above obfervations relative to the greateft height at which clouds are formed in this country in ordinary, it would be about 1 mile; but in fummer they are probably fometimes 1½ mile above the level of the fea. No maximum was given or aimed at by that author's obfervations.

Confidering the great fervice that Dr. Kirwan has rendered to meteorology and chemiftry, and my own obligations to him on thofe accounts, it is unpleafant for me to fignify diffent from the doctrine he inculcates refpecting the ftate of vapour in the atmofphere. At the fame time that his interefting feries of effays in the eighth volume of the Tranfactions of the Royal Irifh Academy were in the prefs, my effays on the force of vapour from water and other liquids, both in a vacuum and in air, and on evaporation, publifhed in the Manchefter Tranfactions, Vol. V. Part 2, were alfo in the prefs. He holds the notion of a chemical folution of water in air; and I maintain that vapour fubfifts in air as it does in *vacuo*, conftituting a peculiar atmofphere, mixing but not combining with any of the gafes of the compound atmofphere. On my principle the denfity of the aqueous atmofphere at any height is totally independent of the denfity of the compound mafs of air, and is to be afcertained by knowing the denfity of vapour at the earth's Obfervations, or fhort ftatement of Dr. Kirwan's and Mr. Dalton's theories of the ftate of atmofpheric vapour.

earth's surface, and its specific gravity; in the same way as we would ascertain the density of the oxygenous or azotic atmospheres, or one of hidrogen, at any given height, having the like *data*.

It has been a matter of surprise to me, that most or all of my essays published in the volume above mentioned, have been copied and circulated in one or other of our periodical publications, except those two just mentioned, which appear to me by far the most important, and which seem too to have been considered as such by the foreign journalists.

I am your's, &c.

J. DALTON.

Manchester, Aug. 22, 1803.

XII.

Cheap and effectual Method of securing Beams of Timber in Houses or elsewhere, which have been injured by the Dry Rot, or are decayed by Time. By Mr. JAMES WOART.*

Easy method of securing decayed timbers in buildings, &c.

WHERE the ends of the girder are decayed by time, or injured by the dry rot, they are often taken out, and new ones put in their place, at a great expense: and if the dry rot is in the walls, the ends of the new girder will be in danger of it again: such was the case at Eltham, in Kent, where in one house there were three new girders to one floor in the space of twenty years; whereas my method will be found infallible, executed at much less expence, and not subject to the dry rot, because the end of the girder may be cut off clear from the wall; and if an air grate is put on the outside, so as to admit air to the end of the girder, it will remain safe from injury.

Plate IX. *Fig*, 2.†—A, shews the end of the decayed girder, with the braces applied upon it.

B B,

* Memoirs of the Society of Arts, 1802. A reward of twenty guineas was awarded to the inventor, who, in an introductory letter states, that by the iron braces, of less cost than 20l. he secured the house of Hannege Legg, Esq. at Putney in Surry, which could not have been done by new beams without loss, derangement, and charges to the amount of eight hundred pounds.

† There are two plates in the Transactions, the second of which forms

Easy method of securing decayed timbers in buildings, &c.

B B, the templets or wall-plates on which the girder refts.

C C C C, one of the iron levers for raifing and fupporting the girder (there being a fimilar one on the oppofite fide.) This lever is moveable on a pin D, which comes through a hole in the lever, diftant about two feet from the end of the girder. This pin forms part of a collar E bedded in the girder. The lever is fix feet long, three inches wide, and three fourths of an inch thick, and extends from the wall-plate along the fide of the girder.

The extremity of the lever is moveable on another pin F, projecting through it from an upright iron G, bedded in the fide of the girder, and carrying a nut and fcrew, which act on a crofs plate H, through which the upright iron paffes.

At the other end of the lever, next the templet, is an iron collar I, bedded in the girder, which collar may be raifed or lowered at pleafure, by means of the nut and fcrew K, forming part of it; and by aid of the cap-plate L, which preffes upon the lever, and alfo clafps it to the girder by its bend at L.

As *Plate* IX. *Fig.* 2, fhows only one fide of the girder, and, as has been before obferved, there being alfo a fimilar lever on the oppofite fide of the girder, their feparate parts, method of connecting them, and their mode of action, are more fully explained in *Plate* VII. *Fig.* 1, 2, 3, where the fame letters are made ufe of to point out the feveral parts.

Fig. 1.—E, fhows the whole of the collar to be bedded in the fide and bottom of the girder, and the pins D D, on which the two levers are moveable.

Fig. 2.—The cap-plate H, the two upright irons G G, with their nuts and fcrews, which act upon the extremities of the two levers by means of their pins F F.

Fig. 3.—The collar I, on which that end of the girder next the templet refts, the fides of which collar are bedded in the girder. C C are the claws or bended legs of the two levers which go into the templet. L is the cap-plate, K K are the nuts and fcrews.

At Mr. Legg's houfe, where the levers above mentioned were applied, the beams of the roof were fo decayed that the roof was in imminent danger, the bearings were entirely rot-

forms Plate VII. of our prefent number. The other unfortunately was neglected to be fent to the engraver's, and the miftake not difcovered till too late. It will be given in our next.

ten,

Eafy method of fecuring decayed timbers in buildings, &c.

ten, and the beams were funk three fourths of an inch, and preffing againft the wall for fupport; if there had not been a large cornice underneath, fupported by brackets, the whole roof muft have fallen.

To put them in order, I firft put fhores or fupports under each end of the two beams, on which the double roof lay, and then forced the four fhores at once, for the fecurity of the roof, the work, and men. The iron levers, C, were then prepared, let into the templet, and fixed on each fide of the beam, on the pins D, projecting from the collar E, bedded in the beam, about two feet from its end. When the whole apparatus was ready, on fcrewing the nuts on the upright irons G, at the extremity of the levers, the beam was raifed to its proper height with great eafe, although it was fuppofed there was above two tons weight on each beam, on account of the lead gutter, and gutter-beam betwixt the double roof, and the rich ornamented ceiling attached to the joift, which was not in the leaft deftroyed except where the iron collar E was fixed, which was put up from the under fide by cutting the ceiling the width of the collar. Thefe beams were fo decayed, and fo hollow, that the common method of bolting plank on each fide of the beam would not have been fafe; and if it could have been executed, the new planks would have been fubject to the dry rot, and the roof ftill in danger, which is now prevented, as the iron is not affected by it. The beam-ends were cut clear from the walls, and the beams are fufpended by means of the iron levers, whofe feet reft on the templets of the walls. An air grate was made, on the outfide of the wall, to admit a current of frefh air to the ends of the timbers. The roof is now much fafer than when originally made, as the timber is fecured from decay; and, owing to the collar E, the bearings are now two feet fhorter at each end of the beam; the bearing on each beam being now, in the whole, four feet fhorter than in its original ftate.

After the beams were brought to their proper height, and the levers and fcrews adjufted, fcrew-bolts were put into the timber, through holes purpofely left in the lever, betwixt D and F, and the whole work thus perfectly fecured.

At the other end of the girder, M, *Plate* IX. is fhown another method of fupporting timbers, where the ends are decayed.

The

The particular irons ufed in this way are fhown in Plate VII. Fig. 4. N is a collar for the girder; O, an iron frame which refts on the templet; P P, two nuts which raife the collar N. R R fhow the clawed ends of the two bars of iron, extending under the girder, bedded therein, and fcrewed to it at their extremities, about five feet diftant from the templet.

Eafy method of fecuring decayed timbers in buildings, &c.

Fig. 5, is one of the iron bars laft mentioned.

S is the claw or lap which projects over the collar N. T is the place where it is fcrewed into the girder.

Fig. 6 and 7. Plate VII. explain a third method of fecuring decayed timbers.

Fig. 6, gives a fide view of a decayed girder: *a*, reprefents the templet; *b*, an iron lever, fix feet long, nearly ftrait, being only cambered one inch, three inches wide, and three quarters of an inch thick; this lever extends along the fide of the girder *c*, and is fecured firmly to it by the fide irons *d d d d*, which are two inches wide, and full half an inch thick, pointed at the ends. The higher ends of thefe fide irons are driven into the girder, and the lower points pafs through holes in the lever into the lower part of the girder, and are held clofe to the girder by ftaples *e e e e*: the fide iron next the templet may be fixed flanting, in order that it may enter founder wood. A claw, *f*, which is part of the lever, refts on the wall plate *a*, and is bedded in it; an iron plate, *g*, lying under the girder, and let into it, paffes through the lever at *h*, connecting it with a fimilar lever on the oppofite fide, and which affifts in the fame way to fupport the girder: *i* is a flooring joift, to fhow how deep the levers are inferted therein.

Fig. 7, fhows the under part of the fame girder; *b b*, are the bottoms of the two levers above mentioned, fixed to the girder by the fide irons and ftaples before defcribed; *k k*, the broad feet of the levers which lie flat upon the wall plate; *f f*, the two claws projecting from the feet, in order to bed in the wall-plate; *i i i i* are joifts, partly cut through, to admit the iron levers to lie clofe to the girder: *g* fhows the iron plate or collar on which the girder bears; it is turned up an inch and a half at each end, to keep the levers clofe to the fides of the girder. This collar fhould be made out of inch-bar iron, with points projecting from it, in the fame manner as the collar at D D, *Fig.* 1, to connect it with the levers, by paffing through holes made through them for that purpofe.

To

To fix the levers, put a shore two feet six inches from the wall, under the girder, to support it; then cut off the decayed end, and take out the templet, or part of the wall plate, if decayed; and put in a stone templet for the irons to rest upon, with mortices in the stone to admit the claws of the lever: then fit the collar underneath the girder, two feet from the wall, to answer the holes in the lever; make an incision in the joists three-fourths of an inch wide, and three inches deep, to admit the levers; fix the levers on each side with the collar, so as to force up the levers together; then with slight shores force up the ends of both levers together, and fix the side-irons firm. The girder will thus be perfectly safe.

The templet or wall plates, on which the levers rest, are made of Portland stone, three feet long, nine inches wide, and five inches deep, with incisions or mortices made therein for the claws of the levers.

Certificates, confirming Mr. Woart's improvements, were received from the commissioners of the navy, from Mr. Joseph Harris, smith, at Putney, and Mr. George Smith, surveyor, at Putney.

XIII.

Account of the Method of extreme Branch Grafting. By the Inventor WILLIAM FAIRMAN, *Esq.**

SIR,

Introductory observations on the useless trees in orchards.

FROM much conversation with Mr. Bucknall on the idea of improving standard fruit-trees, we could not but remark that in apple orchards, even in such as are most valuable, some were to be seen that were stinted and barren, which not only occasioned a loss in the production, but made a break in the rows, and spoiled the beauty and uniformity of the plantation.

To bring these trees into an equal state of bearing, size, and appearance, in a short time, is an object of the greatest importance in the system of orcharding, and also for the

* In a letter to Charles Taylor, Esq. Secretary to the Society of Arts, and inserted in their Memoirs for 1802. For which the silver medal was awarded.

recovery

recovery of old barren trees, which are fallen into decay, not ſo much from age, as from the ſorts of their fruits being of the worn-out and deemed nearly loſt varieties.

Having long entertained theſe thoughts, and been by no means inattentive to the accompliſhment of the deſign, I attempted to change their fruits by a new mode of engrafting, and am bold enough to aſſert that I have moſt fortunately ſucceeded in my experiments; working, if I am to be allowed to ſay it, from the errors of other practitioners, as alſo from thoſe of my own habits. New method of grafting.

My name having ſeveral times appeared in the Tranſactions of the Society for the Encouragement of Arts, &c. and having the honour of being a member of that Society, I thought no pains or expence would be too much for the completion of ſo deſirable an improvement. Under theſe impreſſions, and having many trees of this deſcription, I made an experiment on three of them in March 1798, each being nearly a hundred years old. They were not decayed in their bodies, and but little in their branches. Two of theſe were golden pippins, and the other was a golden rennet. Each likewiſe had been paſt a bearing ſtate for ſeveral years. I alſo followed up the practice on many more the ſucceeding ſpring, and that of the laſt year, to the number of forty at leaſt, in my different plantations *.

The attempt has gone ſo far beyond my moſt ſanguine expectation, that I beg of you, Sir, to introduce the ſyſtem to the Society, for their approbation; and I hope it will deſerve the honour of a place in their valuable Tranſactions.

I directed the proceſs to be conducted as follows: Cut out all the ſpray wood, and make the tree a perfect ſkeleton, leaving all the healthy limbs; then clean the branches, and cut the top of each branch off where it would meaſure in circumference from the ſize of a ſhilling to about that of a crown piece. Some of the branches muſt of courſe be taken off where it is a little larger, and ſome ſmaller, to preſerve the canopy or head of the tree; and it will be neceſſary to take out the branches which croſs others, and obſerve the arms are left to fork off, ſo that no conſiderable opening is to be perceived when you ſtand under the tree, but that they may repreſent an Inſtructions for preparing the trees.

* The average expence I calculated at 2s. 6d. each tree.

uniform head. I muſt here remark to the practitioner, when he is preparing the tree as I directed, that he ſhould leave the branches ſufficiently long to allow of two or three inches to be taken off by the ſaw, that all the ſplintered parts may be removed.

Poſition of the grafts.

The trees being thus prepared, put in one or two grafts at the extremity of each branch; and from this circumſtance I wiſh to have the method called *extreme branch grafting*.

Cement and general preparation and management.

A cement, hereafter deſcribed, muſt be uſed inſtead of clay, and the grafts tied with baſs or ſoft ſtrings. As there was a conſiderable quantity of moſs on the bodies and branches of the trees, I ordered my gardener to ſcrape it off, which is effectually done when they are in a wet ſtate by a ſtubbed birch broom. I then ordered him to bruſh them over with coarſe oil, which invigorated the growth of the tree, acted as a manure to the bark, and made it expand very evidently; the old cracks were ſoon, by this operation, rendered inviſible.

All wounds ſhould be perfectly cleaned out, and the medication applied as deſcribed in the Orchardiſt, p. 14. By the beginning of July the bandages were cut, and the ſhoots from the grafts ſhortened, to prevent them from blowing out. I muſt here, too, obſerve, that all the ſhoots or ſuckers from the tree muſt enjoy the full liberty of growth, till the ſucceeding ſpring, when the greater part muſt be taken out, and few but the grafts ſuffered to remain, except on a branch where the grafts have not taken: in that caſe, leave one or more of the ſuckers, which will take a graft the ſecond year, and make good the deficiency. This was the whole of the proceſs *.

Great advantages of leaving the tree of its full ſize.

By obſerving what is here ſtated, it will appear that the tree remains nearly as large when the operation is finiſhed, as it was before the buſineſs was undertaken; and this is a moſt eſſential circumſtance, as no part of the former vegetation is loſt, which is in health fit to continue for forming the new tree.

—and increaſing its powers.

It is worthy of notice, that when the vivifying rays of the ſun have cauſed the ſap to flow, theſe grafts inducing the fluid through the pores to every part of the tree, will occaſion in-

* The ſyſtem ſucceeds equally well on pear, as alſo on cherry trees, provided the medication is uſed to prevent the cherry tree from gumming.

numerable

numerable ſuckers or ſcions to ſtart through the bark, which, together with the grafts, give ſuch energy to vegetation, that in the courſe of the ſummer the tree will be actually covered over by a thick foliage, which enforces and quickens the due circulation of ſap. Theſe, when combined, fully compel the roots to work for the general benefit of the tree.

The moſt luxuriant fruits preferred.

In theſe experiments I judged it proper to make choice of grafts from the ſorts of fruits which were the moſt luxuriant in their growth, or any new variety, as deſcribed in the ſeventeenth and eighteenth volumes of the Society's Tranſactions, by which means a greater vigour was excited; and if this obſervation is attended to, the practitioner will clearly perceive, from the firſt year's growth, that the grafts would ſoon ſtarve the ſuckers which ſhoot forth below them, if they were ſuffered to remain *. With a view to accompliſh this grand object of improvement, I gave much attention, as I have before obſerved, to the general practice of invigorating old trees; and I happily diſcovered the error of the common mode of engrafting but a ſhort diſtance from the trunk or body, as in Fig. 1. Pl. VIII. There the circumference of the wounds is as large as to require ſeveral grafts which cannot firmly unite and claſp over the ſtumps, and conſequently theſe wounds lay a foundation for after-decay. If that were not the caſe, yet it ſo reduces the ſize of the tree, that it could not recover its former ſtate in many years, and it is dubious if it ever would; whereas, by the method of extreme grafting, as *Fig*. 3, the tree will be larger, in three or four years, than before the operation was performed. For all the large branches remaining, the tree has nothing to make but fruit bearing wood; and from the beautiful verdure it ſoon acquires, and the ſymmetry of the tree, no argument is neceſſary to enforce the practice.

Fig. 2 was my firſt experiment about eight years ſince. The error of No. I was there a little amended, and gave me the idea of engrafting at the extremity. Permit me to remark, that thoſe done in my orchards, on the plan of *Fig*. 2, did not, neither were they able to bear ſo many apples laſt ſeaſon, which was a bearing year, as thoſe on the plan of *Fig*. 3,

* This thought ſhould be kept in ſuſpence, as ten years hence it may appear otherwiſe. However, they will be valuable trees, and highly profitable, as will any other brought under the ſame ſyſtem.

which

which produced me about two bufhels each tree of the fineft fruit I had in my orchards, from the third fummer's wood only. Some engrafted with Ribfton pippins were beautiful.

Approbation of Mr. Bucknall.

Mr. Bucknall vifited me this fummer for the exprefs purpofe of feeing my trees; and he fays the manner of conducting the fyftem is the happieft that ever was conceived. For when a tree has done its beft, and has continued to extreme oldage, juft difpofed to fall into diffolution, as alfo when this is the cafe with trees in a ftagnated and barren ftate, they are thus renovated, and may, with the greateft probability, continue valuable for fifty years to come. I need not fay, do not make the attempt when the energy of growth is over; that will eafily be feen by the body and arms, but more particularly from the fize, figure, fhape, and colour of the leaves, which give the proper indication of health or decay in vegetation.

Should the Society defire it, feveral gentlemen refident here, will gladly fend up certificates to confirm the ftatements.

I remain, SIR,

Your moft obedient fervant,

W. FAIRMAN.

Millers-Houfe near Sittingbourn, Kent, Feb. 9, 1802.

CEMENT FOR ENGRAFTING.

Cement for engrafting.

One	pound of	pitch	To be boiled up together, but not to be ufed till you can bear your finger in it.
One	do. . . .	rofin	
Half	do. . . .	beefwax	
Qtr.	do. . . .	hogflard	
Qtr.	do. . . .	turpentine	

SIR,

Teftimonial of S. D. Bucknall, Efq.

THIS is to certify to the Society for the Encouragement of Arts, &c. that William Fairman, of Millers-Houfe, Lynfted, Efq. has long been a fteady and zealous promoter of the improvement of the ftandard fruits of the country; and that he planted one entire orchard, of fixteen acres, ten years ago.

The fyftem of extreme-branch grafting, now introduced to the public, he has had in contemplation full eight years, though not in its prefent ftyle of fuccefs and elegance; for he has been improving. In thofe operated upon within the laft three

three or four years he has been wonderfully fuccefsful, and I am happy in an opportunity of adding my teftimony to the advantages refulting from this method of renovating old fruit-trees.

An idea equal to the prefent fyftem could not have fallen into better hands than thofe of Mr. Fairman. He is bleffed with a good foil, cultivates the land well, and fteadily attends to improvement. The gentlemen of the committee, by looking at the three little fketches of drawings which reprefent the three trees, will fee that *Fig.* 1 is fo amputated, as not likely to continue in health, fo as again to form a good tree; and that *Fig.* 2 will be many years before, if *ever* it does. But there are now many fine large trees in the ftate of *Fig.* 3, which have been engrafted but three or four years, and yet, as far as ftructure goes, are complete already, and in two years much fine fruit may be expected.

The fyftem is as follows: Make the trees perfectly clean, and keep them as uniformly large as is convenient.

In autumn, 1801, I fpent fome days at Lynfted, and feveral times walked over the plantations with Mr. Fairman, and was very much pleafed with their appearance.

I remain, SIR,

Your obedient Servant,

THOMAS SKIP DYOT BUCKNALL.

February 22, 1802.

Reference to the Engraving of Mr. FAIRMAN'S *Method of Extreme-Branch Grafting; Plate* VIII. *Fig.* 1, 2, 3, 4.

Reference to the engravings.

Figure 1. difplays the old practice, commonly called cleft-grafting.

Fig. 2. Improved experiment on *Fig.* 1, by engrafting higher up the tree.

Fig. 3. Shows the method of extreme-branch grafting, recommended from experience, by Mr. Fairman. Two grafts or fcions are there placed at the extremity of each branch; befides which, additional grafts are inferted in the fides of the branches; as, at AAAAAA, or where they are wanted to form the tree into a handfome fhape.

Fig. 4. Shows upon a larger fcale than the former figures the method of applying the grafts at the extremity of the branches, and retaining them by the bafs-mat bandage and cement.

XIV.

Observations on several Pharmaceutical Preparations, by CIT. STEINACHER, *Druggist at Paris. Abridged by* CITIZEN PARMENTIER*.

Unguentum Nutritum.

Unguentum nutritum.

CITIZEN Dubree, an eminent druggist at Rouen, has lately presented a formula for unguentum nutritum, to the Pharmaceutical Society. As apothecaries zealous for the perfection of their art, have proposed improvements in the preparation of this ointment at different periods, I have thought that an object to which the attention of practitioners has been called from time to time, notwithstanding it is apparently obsolete, deserved a fresh examination †.

The mixture of oil, vinegar, and litharge. Requires carbonic acid, to convert the litharge into carbonate, and thicken the oil. Too much vinegar will prevent the combination. The formula of the French pharmacopœia the best.

When oil, vinegar, and litharge are to be mixed together into a homogeneous mass, a little litharge must be dissolved in acetous acid ‡, and a sufficient quantity of carbonic acid must be introduced, 1st. to convert the greater part of the litharge into white carbonate, which remains diffused through the oil; 2dly, to thicken the oleous mixture, an effect analogous to the thickening of soups by the carbonic acid, with which we were made acquainted by Pelletier. If a sufficient quantity of vinegar to form a saturated saline compound be employed, the mixture will never combine perfectly. This theory, founded on experiment, brings us back to the prescribed formula, as the best that can be adopted, that which produces an ointment, the most bulky, the lightest, and the most cooling to the part affected.

* *Annales de Chimie*, XLVII. 97. (No. 139.)

† Dr. Aikin, the learned editor of Lewis's Materia Medica, says; "The *unguentum nutritum*, made without heat, though now expunged from our dispensatories, is much the best of the ointments prepared from lead, and a very excellent application in many cases. It should not be long kept, but made fresh as wanted." H.

Litharge completely soluble in acetous acid, but not in common vinegar. The residuum of the latter.

‡ Experience has taught me, that levigated litharge is completely soluble in a sufficient quantity of acetous acid, but that the final residuum of its solution in common vinegar, which has been supposed to be superoxided lead, contains only tartrite and malat of lead, with a great deal of extractive matter, which form a paste with a remnant of the litharge reduced toward the metallic state.

affected. It succeeds very well, when the operator is endued with patience, and works in a cold place. It may be abridged however, if, according to the excellent advice of Baumé, we employ coagulated oil of olives, by which the surfaces of contact are increased, and the introduction of the air is facilitated. One important fact with respect to keeping the preparation is, that at the temperature of 15° or 16°, at which most kinds of fermentation take place, a portion of the carbonic acid is extricated, and leaves exposed an oxide at $\frac{8}{100}$, which becomes again yellow. It requires a temperature of 10° to preserve its white colour unaltered.

The process may be shortened by employing coagulated oil.

Warmth spoils it.

But it will keep well in the cold.

Citizen Dubree, and Citizen Granet before him, proposed to expedite the preparation by adding hog's lard; but I find, that this addition diminishes its bulk and levity. In Germany different compositions are made under the name of *nutritum*, as with vinegar of litharge and half its weight of oil of roses, which produce an ointment as white as wax, and of the consistence of a liniment; with vinegar of litharge two parts, and olive oil three parts, which yield a whitish ointment of a moderate consistence; with two parts of olive oil, one part of wax, and two parts of vinegar of litharge, which furnish an ointment of a firm consistence, and a waxy whiteness. But all these compositions are simple mixtures, feebly united, by no means resembling the nutritum of the French shops, and not requiring for their formation a mutual reaction between the different particles of the ingredients.

The addition of hog's lard injurious.

Different compounds called nutritum in Germany.

All these simply mixtures.

Crystallization of Phosphoric Acid.

It is known, that the affinity of phosphoric acid for water overpowers its force of crystallization; in fact this salifiant substance appears commonly in the form of a thick oil. I have lately observed, however, that time, the grand producer of regular crystallizations, effects a symmetrical combination between its particles.

Phosphoric acid commonly a thick fluid; yet crystallizable by time.

I had prepared half a kilogramme of phosphoric acid, according to the method of Lavoisier, with phosphorus and nitric acid, both of them extremely pure. This acid, freed from nitrous gas, reduced to the consistence of a thick syrup, and introduced into a phial with a glass stopper, had been used at different times in the course of a year, without exhibiting any peculiar appearance. The year following I let it remain perfectly

Crystals formed in it by repose.

fectly at reſt in the phial, which was half full, and cloſely ſtopped. After this period I found the ſurface of the fluid covered with a ſaline cruſt, from which ſhot downward priſmatic cryſtals in ſhining laminæ, an inch long, and a line broad, diverging from a centre. I will not deſcribe their geometrical ſtructure, for they are extremely thin, and embedded in a fluid too viſcous for me to take them out without breaking. Beſides, they are ſtill increaſing; laminæ riſe from the bottom of the veſſel, which touch the ſurface of the glaſs, and ſeem preparing to intermix with the ramifications that ſhoot down from the upper ſtratum. The ſides of the veſſel are the ſeat of this beautiful cryſtallization. The centre remains in part concrete or fluid, whence it follows, that if a very regular diſſipation of the particles of the liquid acid of phoſphorus be occaſioned by repoſe, the ſides of the veſſel contribute to it in great meaſure by affording fixed points, to which the poſitions of affinity moſt favourable to cryſtallization direct themſelves.

These cryſtals are priſmatic in thin ſhining laminæ.

Purity of Phoſphorus.

Charcoal combines with phoſphorus during the diſtillation.

Prouſt has informed the public, that, in the diſtillation of phoſphorus, a combination of this ſubſtance with the charcoal conſtantly took place. This important diſcovery extends much farther than its celebrated author has ſhewn.

Phoſphorus purified in Woulfe's method. Muſſin Puſchkin's, or Juch's, ſtill ſhews marks of it.

Take the moſt brilliant and moſt tranſparent phoſphorus, which has not only been ſtrained through chamois leather, according to Woulfe's method, but has alſo been diſſolved ſeveral times in nitro-muriatic acid, as done by Count Muſſin-Puſchkin, or which has been treated with oxigenated muriatic acid, after the mode of Mr. Juch of Wurkburg; let it be heated gently in a long ſlender tube; red parts will ſeparate from it. Put a few grains of this phoſphorus, which is conceived to be ſo pure, on a ſilver ſpoon, and ſet fire to it; a red trace will remain. If the ſpoon be heated in the dark, the red trace will be ſeen ſtill to burn, and a coal will remain impregnated with phoſphoric acid.

Heating with cauſtic alkali incapable of proving the purity of phoſphorus.

Mr. Juch has aſſerted, that his phoſphorus is extremely pure, becauſe it no longer becomes black when heated with cauſtic alkali; but it is in fact becauſe the phoſphure of carbone is unalterable by cauſtic potaſh. According to the indiſputable authority of Prouſt, this re-agent is incapable of proving the purity of phoſphorus. I confeſs, that heated oxigenated muriatic

Heated oxigenated muriatic

muriatic acid destroys part of the carbone of phosphorus, because the combustible power of its oxigen increases in the ratio of its elasticity; but it produces this effect only by burning a proportionate quantity of phosphorus. On the contrary, when it is cold, and its oxigen is reduced to its natural degree of elasticity, it is far from destroying the carbone, it separates it in the state of black oxide, and converts the phosphorus into white oxide, while at the same time, itself returns to the state of simple muriatic acid. I have observed this fact on a stick of transparent phosphorus, which I kept two years in a bottle filled with pure oxigenated muriatic acid, saturated at the temperature of 10°. It is impossible, therefore, to free the phosphorus entirely of charcoal. They oxide, or are acidified nearly in proportional quantities; and though the proportion of charcoal may be diminished, the phosphorus always retains some by its power as a whole. In fine, I am obliged to contradict the assertion of an illustrious master, Citizen Fourcroy, "that we are unacquainted with any direct combination between carbone and phosphorus, though it probably exists," and to consider that product on which chemists have hitherto bestowed the name of pure phosphorus, as a kind of gangue, from which the radical phosphorus is disengaged to enter into a number of combinations, without our being capable of obtaining it in its primitive form.

acid destroys part of the carbone.

Cold separates it in the state of black oxide.

Impossible to free phosphorus from charcoal completely.

Mistake of Fourcroy.

Pure phosphorus yet unknown.

White Oxide of Phosphorus.

When phosphorus is heated in a very long and very slender glass tube, in a sand-heat of 100° of the decimal thermometer, it is covered with a mild light, and exhales a white vapour, which condenses in the upper part of the tube, while, at the same time, part of the phosphorus, with excess of carbone, separates with its red colour. This white vapour, which has acquired for its formation a slight combustion, is a white oxide of phosphorus at a minimum. The following are some of its properties. It is flocculent, possessed of cohesion, and occupies four times the space of the phosphorus employed in the experiment. When it is dry, it does not redden litmus paper. It contains caloric, and inflames on the contact of combustible substances. It powerfully attracts the moisture of the air, and is rapidly converted into phosphorus acid. It differs greatly from the white oxide of phosphorus made by the

Mode of converting phosphorus into a white oxide at a minimum.

Some of its properties.

White oxide of phosphorus at a maximum.

Its properties.

the long action of water, or cold oxigenated muriatic acid. This appears friable and pulverulent. It has lost almost all its latent heat. It is very little inflammable, and does not attract the moisture of the air. It is acidifiable only by the intimate action of an oxigen that contains caloric highly condensed, as that of the nitric acid. In a word, it is phosphorus at a maximum of oxidation.

Regular Crystallization of Essential Oil of Roses.

Regular crystallization of oil of roses.

Citizen Steinacher has lately observed this with attention. He mixed eight kilogrammes of the magna of damask roses (*roses pâles*) with some parts of water, according to the process of Cit. Demachy; and after a day's maceration he drew off by distillation sixteen kilogrammes of water. This was immediately poured into a large glass jar, which was covered with a cloth, and left at rest. In twenty-four hours he found the surface of the water covered with an iridescent pellicle, interspersed with little hexhaedrons, very much resembling the crystals of snow, which the illustrious Cit. Mongé has described. He informs us, that a slight shake is sufficient to tear the crystalline gauze, and reduce it to that irregular form of whitish scales or laminæ, which the oil of roses commonly assumes.

Resembles the crystals of snow,

and requires absolute repose.

SCIENTIFIC NEWS.

Extract of a Letter from Dr. Schaub *to* Mr. Parkinson, *dated Cassel, July* 2, 1803.

I AM busily employed in the analysis of various minerals, the results of which I shall communicate to you in my next. I have noticed among other things also, that the metal called tungsten (Wolfram by the Germans) can only be obtained at the highest degree of de-oxidation, and that this metal does not belong to the class of acidifiable metals; for tungsten cannot be oxidized by means of common processes of oxidation.

Tungsten not acidifiable.

New method of obtaining pure prussic acid.

I have discovered a new method of obtaining prussic acid, in a state of absolute purity. This process consists in pouring upon one part of prussian blue, half a part of sulphuric acid, diluted

 with

with an equal quantity of water, and fubfequent diftillation. The pruffic acid paffes over in the alcohol; its odour greatly refembles the water of the lauro cerafus.

It is a deadly poifon to animals. Perhaps thefe notices may intereft the London chemifts, &c. &c.

Annotation by the Tranflator.

The following method of obtaining tungften, I believe has not been made known in this country. It is recommended by Richter * a German chemift.—F. A.

Richter's method of obtaining tungften.

Let equal parts of tungften oxide (tungftic acid) and dried blood be expofed for fome time to a red-heat in a crucible; pafs the black powder which is formed into another fmaller crucible, and expofe it again to a violent heat in a forge, for at leaft an hour. Tungften will then be found, according to this chemift, in its metallic ftate in the crucible.

Meteoric Stones.

C. BIOT, member of the National Inftitute, in a letter to the French Minifter of the Interior, dated July 20, 1803, gives a detailed account of his inquiries, &c. refpecting a fire ball which exploded in the neighbourhood of Laigle. The memoir will be feparately printed.

Extraordinary meteor and fhower of ftones in France.

On Tuefday, April 26, 1802, about one in the afternoon, the weather being ferene, there was obferved from Caen, Pont-Audemer, and the environs of Alencon, Falaife, and Verneuil, a fiery globe of a very brilliant fplendour, which moved in the atmofphere with great rapidity.

Some moments after there was heard at Laigle, and in the environs of that city in the extent of more than thirty leagues in every direction, a violent explofion, which lafted five or fix minutes.

At firft there were three or four reports like thofe of a cannon, followed by a kind of difcharge which refembled a firing of mufketry; after which there was heard a dreadful rumbling like the beating of a drum. The air was calm and the fky ferene, except a few clouds, fuch as are frequently obferved.

* Richter ueber die neuen gegenftande der Chimie. Part I. p. 49.

The

The noiſe proceeded from a ſmall cloud which had a rectangular form, the largeſt ſide being in a direction from eaſt to weſt. It appeared motionleſs all the time that the phenomenon laſted. But the vapour of which it was compoſed was projected momentarily from the different ſides by the effect of the ſucceſſive exploſions. This cloud was about half a league to the north-north-eaſt of the town of Laigle; it was at a great elevation in the atmoſphere, for the inhabitants of two hamlets a league diſtant from each other ſaw it at the ſame time above their heads. In the whole canton over which this cloud hovered, a hiſſing noiſe like that of a ſtone diſcharged from a ſling was heard, and a multitude of mineral maſſes, exactly ſimilar to thoſe diſtinguiſhed by the name of *meteoric ſtones*, were ſeen to fall at the ſame time.

The diſtrict in which the ſtones fell forms an elliptical extent of about two leagues and a half in length and nearly one in breadth, the greateſt dimenſion being in a direction from ſouth-eaſt to north-weſt, forming a declination of about 22°. This direction which the meteor muſt have followed is exactly that of the magnetic meridian; which is a remarkable reſult.

The largeſt of theſe ſtones fell at the ſouth-eaſt extremity of the large axis of the ellipſe; the middle-ſized ones fell in the centre, and the ſmalleſt at the other extremity. It thereby appears that the largeſt fell firſt, as might naturally be ſuppoſed.

The largeſt of all thoſe which fell weigh 17½ pounds. The ſmalleſt he ſaw weighed about two gros, which is the thouſandth part of the former. The number that fell is certainly *above* two or three thouſand. They were friable ſome days after their fall, and ſmelled ſtrongly of ſulphur. Their preſent hardneſs was acquired gradually.

Abſtract of a Memoir on the Febrifuge Principle of Cinchona, by CIT. SEGUIN *.

THE object propoſed to himſelf by the author in the taſk he undertook was, to point out the means of knowing with certainty the true febrifuge principle of cinchona, to diſtinguiſh the ſpecies that contain it from thoſe that do not, and laſtly to appreciate its quantity and quality.

* Bulletin des Sciences, No. 77.

Hitherto

Hitherto the sight and taste have been the only tests of the presumable qualities of the peruvian bark of the shops; but as these have no precise standard, and are inapplicable to powdered bark, they very imperfectly indicate the presence of the febrifuge principle. It was of importance, therefore, to substitute to these means, little better than illusory, others not only capable of calculation, but likewise invariable. Chemical re-agents alone can answer these ends. Sight and taste insufficient to judge of the goodness of bark.

In consequence Cit. Seguin began by isolating the respective properties of all medicinal substances, and he examined the action they exert on all other chemical substances.

These researches led him to develope very decisive characteristics in the febrifuge principle of cinchona; which place it in a perfectly distinct class. The following are its characters. The febrifuge principle of bark very distinct.

It precipitates the solution of tan, but not the solutions of gelatine and sulphate of iron. Its characters.

When cinchona has not all these characters, it is a proof that it is mixed with something else, or that it does not contain the febrifuge principle.

The author has subjected to this analysis all the known species of cinchona, found among all the druggists and apothecaries of Paris and Versailles, and constantly obtained the same results. Various specimens of bark subjected to this test,

Unfortunately these researches have shown, that but an infinitely small quantity of good, unmixed cinchona, is to be procured in the shops; the greater part being either destitute of the febrifuge principle, or mixed, or of a very inferior quality, though containing no mixture. which proved that very little good is to be found in the shops.

These results are of so much the greater importance, because the effects of different kinds of cinchona in fevers are only in proportion to the greater or less quantity of the febrifuge principle they contain; and those which contain none, as well as all the substances that may be mixed with them, are more or less injurious to the system. Efficacy of cinchona proportionate to the quantity of febrifuge principle it contains.

The experiments of Cit. Seguin on the febrifuge principle of cinchona, having convinced him that most of the bark found in the shops was injurious or inefficacious, because it was spoiled by keeping, adulterated by mixture, or deprived of the febrifuge principle; he has endeavoured to obtain a febrifuge principle always the same, more efficacious, more certain in its effects, more capable of assimilation with our system, and so cheap, that there could be no temptation to adulterate it. Defects of the common bark of the shops.

 To

To attain this important object, the author has inquired what the true caufe of fevers, as of their effects, is; what the nature of the febrifuge principle of chinchoa, and what its action on our fyftem. He has fubjected to the action of the re-agents pointed out for the febrifuge principle of cinchona, all chemical and medicinal fubftances; and affured himfelf, whether fuch of thefe fubftances, as might contain the febrifuge principle, did not contain, at the fame time, other fubftances prejudicial to the animal economy. Laftly, he had to cure fevers by the help of thefe remedies, and then confirm this theory by repeated experiments. Such is the courfe Cit. Seguin has purfued.

The febrifuge principle is gelatine.

The new febrifuge principle, which he propofes to fubftitute inftead of cinchona, becaufe it unites all the advantages of the bark, without any of its inconveniences, is gelatine in its pure ftate.

Advantages this poffeffes over bark.

Confidered in a medical, economical, and political view, gelatine promifes much greater advantages than bark, in its application to the cure of fevers. It occafions no irritation; procures quiet fleep and gentle perfpiration; keeps the belly open, without producing colic or naufea: has no unpleafant flavour; reftores the ftrength, and is digefted even by the weakeft ftomach, that would reject the bark as foon as adminiftered.

Difadvantages of bark.

On the other hand, cinchona irritates the fyftem, difturbs the fleep, has a difagreeable tafte, frequently occafions coftivenefs, and is very indigeftible.

Comparative cheapnefs of gelatine.

In an economical view, there is ftill greater difference between cinchona and gelatine; the price of the latter being to that of the former at moft as one to thirty-two.

Laftly, gelatine is indigenous, cinchona is not; and the purchafe of the latter requires us to fend abroad a very confiderable fum of money, which might be kept at home by adopting the ufe of gelatine.

Cafes cured by it.

To this memoir the author has fubjoined thirty-feven cafes, in which he performed a cure with gelatine, under the eyes of fome refpectable phyficians, and he has defired a Committee to be appointed, to repeat his experiments, and report upon them.

Committee appointed to examine its effects.

Accordingly Citizen Portal, Defeffarts, Hallé, Fourcroy, Berthollet, and Deyeux, have been nominated for this purpofe.

 Their

Their experiments are made at the School of Medicine, in a room exclufively appropriated to thefe inquiries; already a great number of patients have been cured; and the Committee will foon make their firft report on thefe cafes. which appear to be confirmed by experience.

Query by a Correfpondent refpecting the Auguftine Earth.

To Mr. NICHOLSON,

SIR,

WE poffefs many excellent elementary works on Chemiftry, both original and tranflations, fuch as Thompfon, Accum, Murray, Henry, Parkinfon, La Grange, Green, Fourcroy, Jacquin, &c. but in none of thefe authors is mentioned the method for obtaining the new earth, called *Auguftine*; although moft of thefe works have been publifhed a confiderable time after this earth was made known by the German difcoverer, Profeffor Tromfdorf. I have alfo made enquiry, concerning this fubject, of moft of the public teachers of chemiftry, and other individuals, who ftand high as chemical philofophers, but in vain; I will therefore thank you to allow thefe lines a place in your valuable Journal. Perhaps one of your Correfpondents will be kind enough to favour me with the procefs for obtaining this earth; for the author of thefe lines cannot find it in the mineral which is faid to contain it; having purfued the ufual methods of examining mineral fubftances for that purpofe. Auguftine earth.

I am,

SIR,

Your's, &c.

P. O.

Spaniard faid to refift high Degrees of Heat and ftrong chemical Agents.

THE prints of Paris, and fome of our own, too implicitly copying them, have for fome time exhibited a ftrange narrative of a young Spaniard, born at Tolofa, and now 23 years of age, of whom it has been very particularly affirmed, Extraordinary ftory of a Spaniard faid to refift heat and corrofive acids.

That

That though his skin exhibited no appearance of peculiarity, either natural, or indicating preparation by art, yet without injury, 1. He bathed his feet for six minutes, and washed his hands and face in oil heated to 250° of Fahrenheit, which is 38 degrees hotter than boiling water. 2. He did the same with a solution of sea salt, heated 12 degrees higher. 3. He stood with his naked feet upon a bar of iron near the welding, or at the white heat; he held the bar in this state in his hands, and rubbed it on the surface of his tongue. 5. He washed his mouth with the strongest sulphuric and nitric acids, and applied the same to the other parts of his skin, with no other effect than that the nitric acid produced a yellow tinge; and 6. he remained a considerable time in an oven heated to within 18 degrees of the boiling water point.

Though our reasoning from analogy in matters of experiment, is liable to mislead, as well by infusing too much doubt as too much confidence, yet I should have passed over this tale without notice, if I had not heard of it from very respectable correspondents. I suppose there may be something extraordinary in the degree of insensibility of the subject in question, as the Institute has paid attention to him; but I understand that the story is now told with great abatements. Citizen Pinel, a man of information, and well known as an accurate observer, is commissioned to report upon the same; and I have no doubt but his account will shew how much easier it is for men to tell falsehoods than to reverse the course of nature.

---

*Method of giving Malt Spirits the Flavour of Brandy.**

Flavour of malt spirits amended.

INTO a quart of malt spirits put three ounces and a half of finely powdered charcoal, and four ounces and a half of ground rice. Let these ingredients remain during fifteen days, only observing to stir them often: at the expiration of this time, let the liquor be strained, and it will be found to be much improved.

Preparation of a Lute proper for Chemical Operations. By C. PAYSSE, *Professor of Chemistry.*†

Lute, eggs and chalk, or slaked lime.

IN the preparation of the oxigenated muriatic acid in the large way, the necessity I found for a lute, which, to the advantage

* From the Bibliotheque Physique Economique, No. 10. An. XI.
† From the Annales de Chimie, No. 137. An. XI.

vantage of being cheap, should add those of being easily prepared; of resisting the action of the destructive vapour of the acid, as well as the strong heat which the luted part is often required to bear; which should be easy of application, and in an uniform manner, and not harden too quickly; obliged me to make some experiments on the subject, the result of which have been very satisfactory.

After making a great number of mixtures with different substances, I made choice of the following, which produced me a homogeneous composition, drying as slowly as could be desired, extremely hard; when dry, of a very compact texture; and, in short, possessing all the properties I had desired.

Take the white of two eggs, with their yolks, and of powdered carbonate of lime, or of quick lime well slaked in the air, about half the weight of the eggs; spread it on a cloth, and apply it as a lute.

NOTE.

THIS lute, the composition of which is very simple, possesses a degree of elasticity, when dry; I have formed vessels of it, which are impermeable to water, and susceptible of being polished on the wheel. This composition resembles the substance of which the pipes, called *Meerschaum*, are made.

---

*Two new Quadrupeds.**

Two living Quadrupeds have lately been received at the Museum of Natural History at Paris, which are entirely unknown among naturalists, and were brought to Europe by Captain Baudin. Professor Geoffroy (of Egypt) who has inserted a description of them in the annals of the Museum, calls them Fascolomes. They come from the western coast of New Holland; their fur promises to be of some use; and, according to the opinion of Captain Hamelin and his suite, their flesh is very excellent food. They are particularly interesting to naturalists from the singularity of their organization. In the form of the head, the number, arrangement, and nature of the teeth, and the form of the fore feet with which

Two new Quadrupeds from New Holland.

* Decade Philos. No. 51. An. XI.

they

they burrow in the earth, they resemble the marmot; but they differ from them, by the female having a pouch beneath the belly, and by the whole structure of the organs of generation, in which they are similar to the *sarique* of Buffon. The form of the hinder foot is the same as in that animal with a pouch; the thumb being distinct from the other fingers, and without a nail: the tail is so short, that it remains hid bebelow the hair, which is brown, bushy, and very long. The Fascolomes of the Menagerie are yet young, but are already larger than rabbits. Their temper is admirable; they may be handled, or removed, without shewing any symptoms of fear, anger, or uneasiness; their movements are heavy and clumsy; they live under ground, sleep during the day, and go in search of food at night. In general they possess but little energy or activity; they scratch themselves like the monkey, and they may be fed with bread, milk, roots, and every sort of herbage.

Preservation of Iron from Rust.

Defence of iron from rust.

CIT. Conté has adopted a method, which he finds effectual, for preventing the oxidation of iron and steel; or, in popular terms, to prevent iron and steel from rusting. It consists in mixing with fat oil varnish, at least half, or at most four-fifths of its quantity of highly rectified spirits of turpentine. This varnish must be lightly and evenly applied with a sponge; after which the article is left to dry in some situation not exposed to dust. He affirms, that articles thus varnished retain their metallic lustre, and do not contract any spots of rust. This varnish may also be applied to copper, of which it preserves the polish, and heightens the colour. It may be employed with particular advantage to preserve philosophical instruments from any change, in experiments where, by being placed in contact with water, they are subject to lose that polish and precision of form which constituted part of their value.

ACCOUNT

ACCOUNT OF NEW BOOKS.

Philosophical Transactions of the Royal Society of London, for the Year 1803. Part I.

THE Contents of this Part are, 1. The Bakerian Lecture, Observations on the Quantity of horizontal Refraction; with a Method of measuring the Dip at Sea. By William Hyde Wollaston, M. D. F. R. S. 2. A Chemical Analysis of some Calamines. By James Smithson, Esq. F. R. S. 3. Experiments on the Quantity of Gases absorbed by Water at different Temperatures and under different Pressures. By Mr. William Henry. 4. Experiments and Observations on the various Alloys, on the Specific Gravity, and on the comparative Wear of Gold. Being the Substance of a Report made to the Right Honourable the Lords of the Committee of the Privy Council, appointed to take into Consideration the State of the Coin of this Kingdom, and the present Establishment and Constitution of His Majesty's Mint. By Charles Hatchett, Esq. F. R. S. 5. Observations on the Chemical Nature of the Humour of the Eye. By Richard Chenevix, Esq. F. R. S. and M. R. I. A. 6. An Account of some Stones said to have fallen on the Earth in France, and a Lump of Native Iron said to have fallen in India. By the Right Honourable Charles Greville, F. R. S. 7. Observations on the Structure of the Tongue, illustrated by Cases in which a portion of that Organ has been removed by Ligature. By Everard Home, Esq. F. R. S. 8. Observations on the Transit of Mercury over the Disk of the Sun; to which is added an Investigation of the Causes which often prevent the proper Action of Mirrors. By William Herschell, LL. D. F. R. S. &c. 9. An Account of some Experiments and Observations on the Constituent Parts of certain Astringent Vegetables; and on their Operation in Tanning. By Humphry Davy, Esq. Professor of Chemistry in the Royal Institution. 10. Appendix to Mr. William Henry's Paper, on the Quantity of Gases absorbed by Water, at different Temperatures, and under different Pressures.*

APPENDIX.

Meteorological Journal kept at the Apartments of the Royal Society, by Order of the President and Council.

* Our Readers will observe, that we have as usual, reprinted in our Journal most of these valuable Papers.

An

An Essay on the Law of Patents for new Inventions; to which are prefixed Two Chapters on the general History of Monopolies, and on their Introduction and Progress in England, to the Time of the Inter-regnum; with an Appendix containing Copies of the Caveat, Petition, Oath, and other Formulæ, with an arranged Catalogue of all the Patents granted from the 1st of January 1800, to the present Time. By JOHN DYER COLLIER. 1803. Longman and Rees, Royal 8vo.

One of the most obvious expedients for taxing the industry of man in social life, but at the same time one of the most pernicious, consists in monopolies. Accordingly we find in all governments that this resource is more or less adopted, and trades, manufactures, and various operations, become confined to the excutive power, or what is worse, to the private favourites of men of influence. A long series of years have elapsed since this nuisance was abolished in our country, by the statute of James, and the monopolies that yet remain, are under the direct sanction of law, and so few, that a common observer would be disposed to say we have none.

A class of monopolies which has constituted the subject of a clause of exception in that act, consists in the sole working and making of new manufactures for a limited time under royal grant, to the first and true inventor thereof. It has been a subject of discussion whether even this exclusive privilege which is often made the instrument of public deception, and sometimes of oppression by wealthy Individuals to crush the industry of ingenious men by expensive legal processes under letters patent, for objects of public possession; it has often been disputed whether this exclusive privilege be a benefit or an evil. The facts I think are, that many private fortunes are lost, in supporting pretended inventors, or in bringing real ones into effect, and that our arts, trade and sciences are greatly benefitted by this last operation:—

The subject of patents and monopolies in general is therefore of great interest and practical importance, and I have no doubt the public will receive this compendium as a valuable addition to their means of information respecting them.

Electrical Calamine.

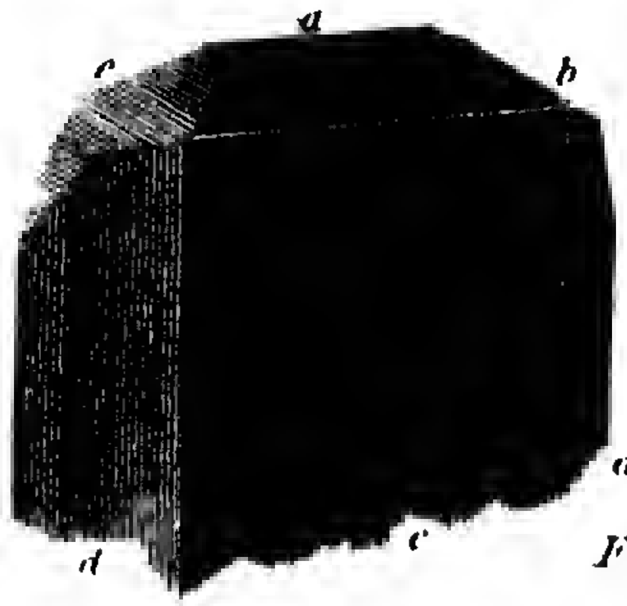
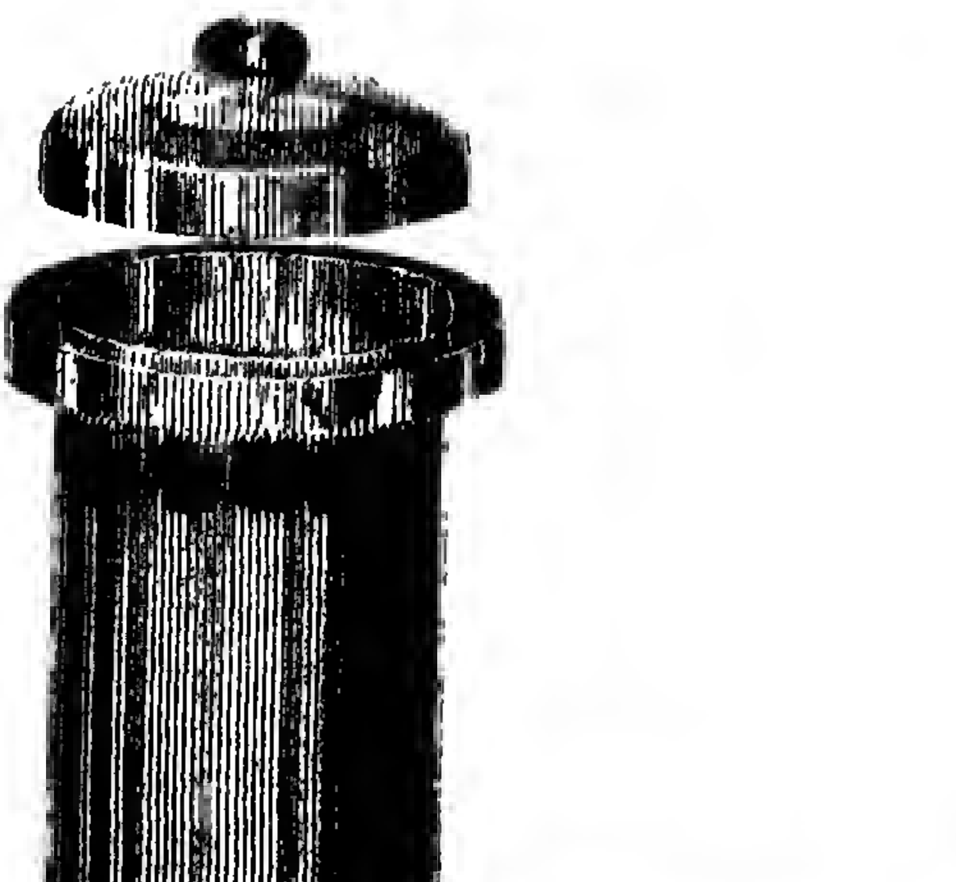


Fig. 1.

Fig. 2.

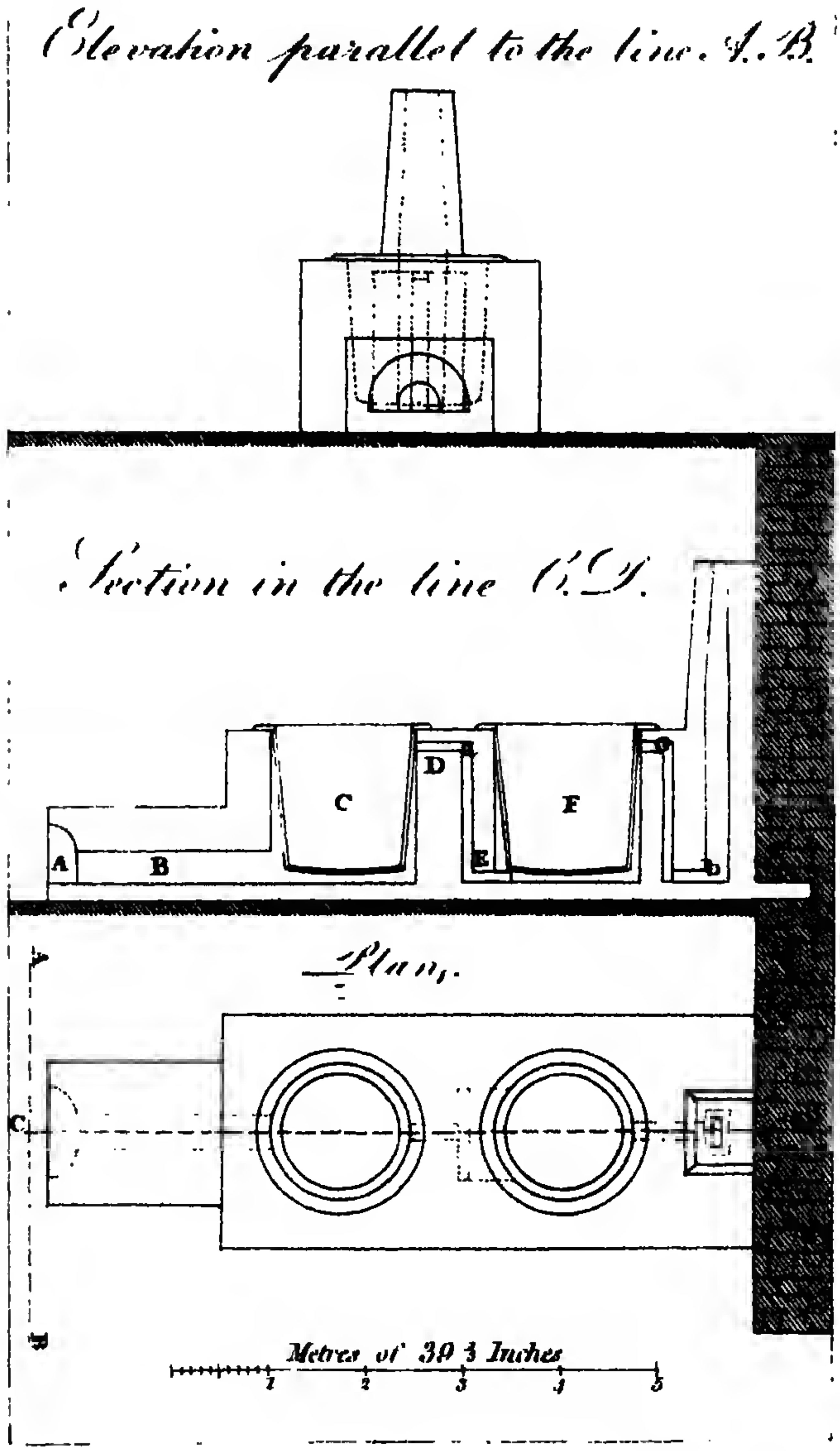


Closure of Vessels
for Anatomical Preparations &c.

Blunt del.

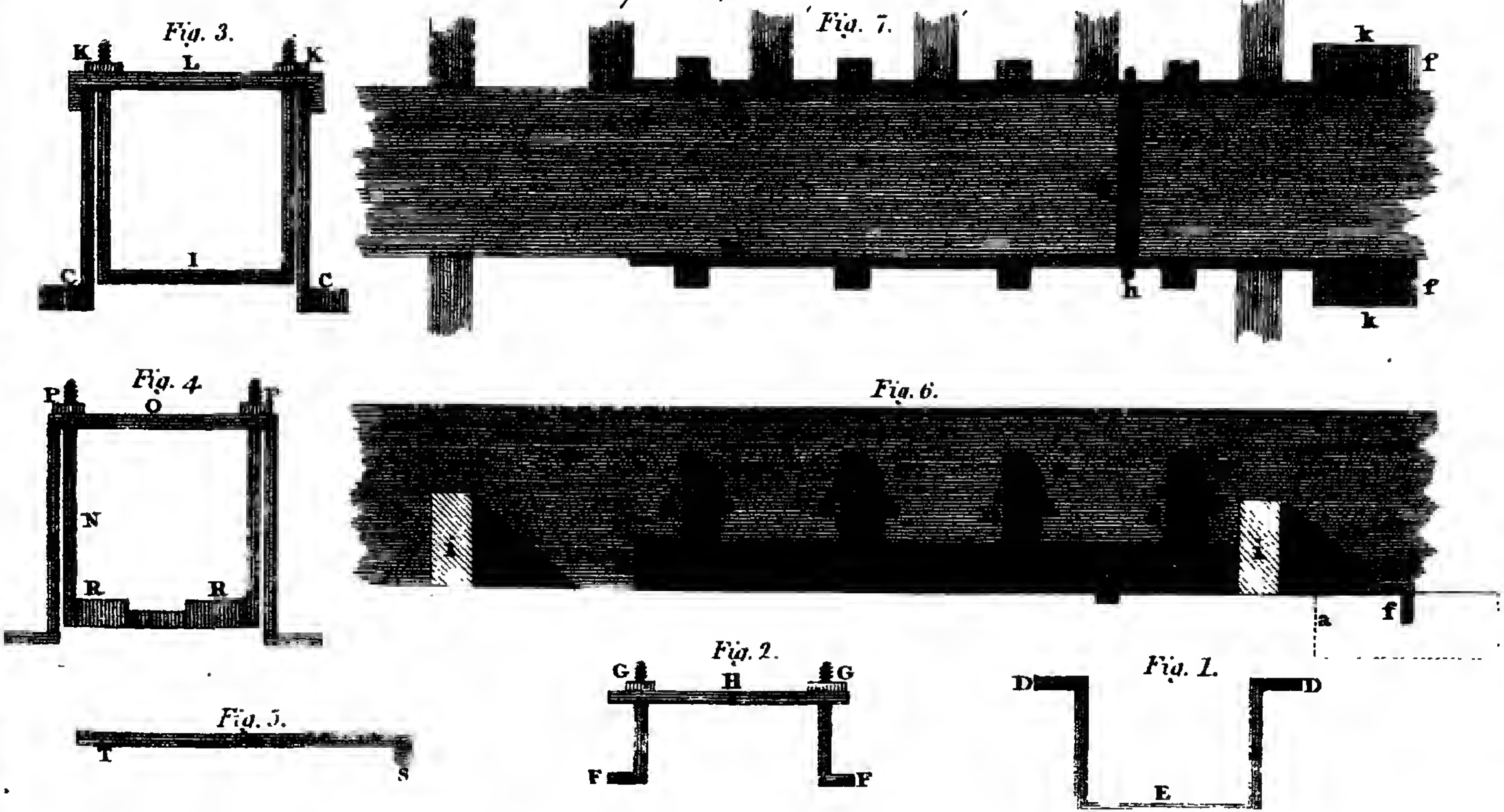
Mutlow [illegible]

Furnace for Evaporation by regulated Temperature.



Mutlow sc. Russell Co.

Mr. Weart's method of supporting Decayed Timbers.



Mutlow Sc. Russ.

Mr. Wm. Fairman's Method of Extreme Branch Grafting.

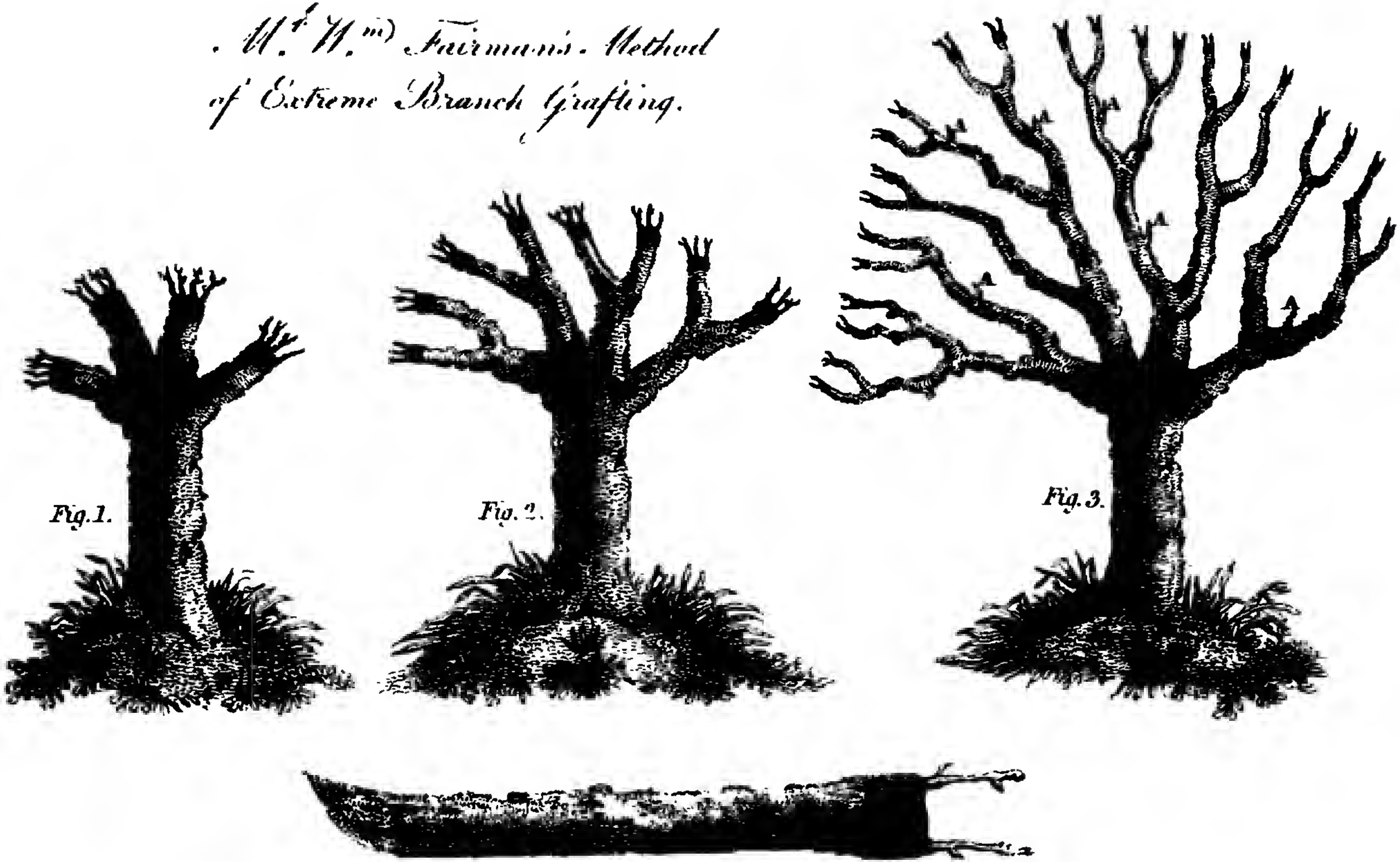


Fig. 1. Fig. 2. Fig. 3. Fig

A
JOURNAL
OF
NATURAL PHILOSOPHY, CHEMISTRY,
AND
THE ARTS.

NOVEMBER, 1803.

ARTICLE I.

Experiments and Obſervations on the various Alloys, on the Specific Gravity, and on the comparative Wear of Gold. Abſtracted from the Memoir of CHARLES HATCHETT, *Eſq.* F. R. S. *in the Philoſ. Tranſ. for* 1803.

(Concluded from Vol. V. Page 303 *of our Journal.)*

Experiment I.

TWELVE pieces of ſtandard gold were firſt examined, and were placed ſo that ſix were oppoſed to ſix. Loſs of ſtandard gold by friction.

The braſs frame, in which each upper piece was fixed, weighed 1604 grains; and it was found neceſſary to add to each a weight of lead, equal to 19825 grains; ſo that the pieces were rubbed againſt each other under the preſſure of $19825+1604=21429$ grains $=$ 3 lb. 8 oz. 12 dts. 21 grs *.

* This weight was not employed till repeated trials had proved the extreme difficulty, and almoſt impoſſibility, of producing any perceptible effect with leſs, in a moderate period of time; and, even with this weight, the experiments were found to be exceedingly tedious. The only evil which reſulted from ſuch a preſſure was, that the comparative wear of the fine gold appeared much more conſiderable than would have been the caſe, if a ſmall weight could have been employed; ſome obſervations will therefore be found in the ſubſequent pages, which point out the neceſſity of making an allowance for this circumſtance.

The machine was then put in motion, until the index ſhowed that 286690 revolutions had been performed; and, as a double crank acted during each revolution, the pieces were rubbed againſt each other alternately, in oppoſite directions, 573380 times, being twice the number of the revolutions.

The twelve pieces of ſtandard gold, being taken out, were weighed, and were found to have loſt 8,60 grs.

Experiment II.

Of gold alloyed with an unequal weight of copper.

Twelve pieces of gold combined with an equal proportion of copper, and without any impreſſion, loſt 103,11 grs. in 70340 revolutions.

Experiment III.

Of fine copper.

Twelve ſimilar pieces of fine copper, loſt 174,80 grs in 22200 revolutions.

Hence it appears, that ſtandard gold loſes leſs by friction than gold much debaſed by copper, and this leſs than copper alone.

Gold alloyed with copper, ſilver, tin, and iron.

The next ſeries of experiments was made with gold differently alloyed; when gold 1. made ſtandard by copper, 2. reduced to 18 carats by copper, 3. made ſtandard by copper and ſilver, 4. made ſtandard by ſilver, 5. of 23 car. $3\frac{3}{4}$ grs. fine, 6. made ſtandard by tin and copper, 7. made ſtandard by iron and copper, 8. alloyed with an equal quantity of copper, was found to have loſt in the following proportions, in the order in which they have been enumerated. 0 grs. 0 grs. 0,10 grs. 0,10 grs. 4,20 grs. 15,30 grs. 21,60 grs. 65,78 grs. The wear of the pieces alloyed with equal parts of gold and copper, and with iron and copper, was ſo rapid, that they were obliged to be taken out of the machine after 105480 revolutions; and thoſe containing tin were worn ſo thin in 189000 revolutions, as to require being removed: the reſt ſuſtained 200300 revolutions, whence their comparative loſs was ſtill leſs than as above given.

Gold ſimilarly alloyed and ſtamped.

This experiment being made on ſmooth, flat pieces, it was repeated with others of ſimilar compoſition ſtamped with the die before deſcribed, only omitting the compound of equal parts of gold and copper, and adding pieces of ſtandard ſilver and of fine copper. The number of revolutions were only 20680, and the pieces, taking them in the order already mentioned, now loſt reſpectively: 0,60 grs. 4,80 grs. 1,20 grs.

3,50

3,50 grs. 4,60 grs. 13,80 grs. 7,60 grs. The ſtandard ſilver loſt 3,70 grs. the fine copper 46,30. **Standard ſilver and fine copper.**

From comparing the effects produced with the number of revolutions, it is obvious, that much more is loſt in the friction of emboſſed ſurfaces, than of plain. **raiſed ſurfaces loſe moſt.**

The experiments were afterwards varied by placing pieces of the different compoſitions in ſuch a manner, that in ſome caſes the friction ſhould be between ſimilar pieces, in others, between thoſe differently alloyed. The reſults of all thoſe experiments are tabulated, but they would occupy too much room to enter into them minutely; and though, from unavoidable circumſtances, ſome little inaccuracies occaſionally occurred, they may be concluded, as Mr. H. obſerves to prove: **Friction between ſimilar and diſſimilar pieces.**

1ſt. That fine gold, or of 23 car. 3¾ grs. when expoſed to friction againſt gold of an equal quality, under the preſſure of a conſiderable weight, ſuffers a very notable loſs; and, although various circumſtances ſeemed to indicate, that but little effect, in reſpect to abraſion, is produced under a leſs weight, yet it muſt be remembered, that the firſt caſe may occur.* **Fine gold.**

Moreover, that fine gold, under all circumſtances, is more ſubject to have any raiſed parts of its ſurface obliterated, than any variety of alloyed gold; not always, nor indeed ſo much,

* It is proper to remark, that the preceding experiments were made under a much greater weight than can be ſuppoſed to operate generally during the circulation of money; and as, by ſome previous experiments, a leſs weight was found to produce, during a certain time, little or no effect, it may be ſuſpected, that although, under a greater preſſure, fine or very ductile gold ſuſtains a greater loſs than ſome of thoſe which are reduced to ſtandard, yet, under a leſs preſſure, or ſuch as that which moſt commonly prevails in the courſe of the uſual wear of coin, the reverſe may probably be the caſe; for then the ſame cauſes operate with leſs rapidity, during a long period of time. From many various circumſtances, there is reaſon therefore to believe, that the wear of coin againſt coin of a ſimilar quality is, under a ſmall or very moderate weight, in the inverſe ratio to the degree of ductility; but this is only to be underſtood in the abovementioned caſe, of coin rubbed againſt coin of equal quality. **Wear of coin in the inverſe ratio of its ductility.**

by

by actual abrafion, as by having the protuberant parts preffed and rubbed into the mafs, in confequence of its extreme foftnefs and ductility.*

2d. That fine gold, or of 23 car. $3\frac{3}{4}$ grs. when rubbed againft the various kinds of alloyed gold, always or generally fuffers the greateft comparative lofs.

Standard gold.

3d. That gold reduced to 22 carats, or to ftandard, by filver, or by filver and copper, or merely by copper, fuffers by friction, under general and fimilar circumftances, a fmaller diminution than the fine gold abovementioned; and, with or without abrafion, the protuberant parts on the furfaces of thefe pieces remain much more permanent, under all circumftances, than thofe of the fine gold. The difference of wear between the three kinds of ftandard gold abovementioned, does not in reality appear to be very confiderable; but, upon the whole, the preference may be given to gold alloyed with a mixture of filver and copper, or to that which has only copper for the alloy.

Gold with iron or tin.

4th. That gold made ftandard partly by the addition of iron or tin, fuftains a greater lofs by friction than either of the three kinds of ftandard gold above-mentioned.

18 carat gold, with copper.

5th. That gold reduced to 18 carats by copper, is more liable occafionally to wear, in a fmall degree, than the three kinds of ftandard gold which have been lately mentioned, provided that the friction takes place between pieces of equal quality; but, in the contrary cafe, the principal lofs always falls on the foft or ftandard gold, when it is oppofed to gold of 18 carats, which is confiderably harder.

Gold much debafed with copper.

6th. That gold more debafed than that of 18 carats, fuch as gold alloyed with an equal proportion of copper, fuffers very confiderably more than any of the kinds hitherto mentioned, provided that the pieces are of the fame quality; but, on the contrary, fine and ftandard gold experience a very great lofs, when expofed to the action of this debafed gold, while the lofs of the latter is comparatively much lefs.

Difadvantage of foftnefs in coin.

* This is, however, of much confequence; for, although coin may not fuffer by actual abrafion, yet, if the impreffion made upon it can fo foon be deftroyed, it follows of courfe, that the pieces become (although ftill allowed to be current) no better than mere blanks, or fragments of a bar or ingot.

7th

7th. That the wear of ſtandard ſilver appears to be nearly equal with that of fine gold; but more than that of gold made ſtandard by ſilver or by copper, and leſs than that of gold much debaſed by copper. Standard ſilver.

8th. That, as gold which is not inferior to ſtandard wears in general leſs than ſtandard ſilver, ſo does this laſt ſuffer much leſs than copper.

The loſs ſuſtained by copper, when rubbed againſt copper, is infinitely more than that of the former metals; and, when theſe are expoſed to the action of copper, they, as well as the copper, ſuffer a very conſiderable loſs. This appears from the general reſults of theſe experiments, which prove, that pieces of metal which are the moſt ſubject to wear, are thoſe which produce the greateſt loſs upon other pieces of metal, when rubbed againſt them; and it is remarkable, that *in ſuch a caſe*, the loſs does not always fall on one in preference to the other; ſo that the wear can only be conſidered in the aggregate, although one of the pieces may be regarded as the principal cauſe. Copper

In order, however, to illuſtrate the reſults of the preceding experiments, as far as they concern the ſofter and harder kinds of ſtandard gold, and to aſcertain more fully the comparative wear of flat and ſmooth ſurfaces with that of ſuch as were partly protuberant, an experiment was made, with two kinds of ſtandard gold: 1ſt. Gold made ſtandard by fine Swediſh copper, which was very ductile; and, 2d. Gold made ſtandard by a mixture of fine Swediſh copper and dollar copper. This was as brittle as was compatible with rolling and ſtamping; and was prepared by melting gold made ſtandard by fine Swediſh copper, with an equal quantity of gold rendered brittle by the ſtandard proportion of Swediſh dollar copper, which was mentioned in the firſt ſection of this paper. Gold made ſtandard by different kinds of copper.

It may here be obſerved, that a diſtinction muſt be made between hard and brittle metal. If a metal is diſpoſed to crack when rolled, without requiring any extraordinary force to enable it to paſs the rollers, then it may be regarded as brittle; but, if it requires an extraordinary force to make it paſs the rollers, and is not diſpoſed to crack, then it may be conſidered as hard. Diſtinction between hard and brittle metal.

This

This experiment proves,

Very ductile and harder gold compared.

1ft. That very ductile ftandard gold, when expofed to the friction of gold of a fimilar quality, fuffers lefs by abrafion than gold which is comparatively brittle, or harder, and which is fubjected to friction under the fame circumftances.

2d. That when foft gold and brittle or hard gold rub againft each other, the greateft lofs is fuftained by the foft gold. And,

3d. That pieces which have raifed or emboffed furfaces, fuffer a greater lofs, under every circumftance, than thofe which are fmooth and flat.

Coin rubbing againft coin, in ordinary circulation, lofes but little.

The whole of the foregoing experiments were made with the machine called No. 1; and, as the friction was continued, in each experiment, during many days, with a preffure upon each couple of pieces equal to 3 lbs. 8 oz. 12 dts. and 21 grs., and as (confidering the feverity of fuch a trial) the lofs fuftained by the pieces, feparately or collectively, was not very confiderable, it may with reafon be inferred, that ftandard gold does not eafily fuffer abrafion by the friction of metal againft metal, or of coin againft coin, efpecially under the circumftances which commonly prevail during the circulation of money.

In the machine No. 1, the pieces of gold were oppofed face to face; it now therefore appeared proper, that the facts thus afcertained concerning the wear of gold, of different degrees of ductility, fhould be farther examined, and corroborated by a different method. To effect this, the fecond of the machines before defcribed, was employed.

Experiments with machine, No. 2.

Two hundred pieces of gold, of five different qualities, were employed in this experiment; twenty pieces of each kind were plain and fmooth, the others were ftamped with the die already mentioned. The two hundred pieces were mingled, and were enclofed within the cubic box.

Different qualities of the gold.

The following were the qualities of the gold. 1. Gold of 23 car. $3\frac{1}{4}$ grs. 2. Gold made ftandard by filver. 3. Gold made ftandard by filver and copper. 4. Gold made ftandard by fine Swedifh copper. 5. Gold made ftandard by equal parts of fine Swedifh copper and dollar copper.

Their lofs.

After 71720 revolutions of this machine, performed in 40 hours, the lofs fuftained was found to be as follows: of No. 1. the

the unftamped pieces 92,8 grs. ftamped, 95,6 grs. No. 2, unftamped, 63,5 grs. ftamped, 60,1 grs. No. 3, unftamped, 12 grs. ftamped, 11,7 grs. No. 4, unftamped, 18 grs. ftamped, 19,2 grs. No. 5, unftamped, 13 grs. ftamped 12,1 grs. The total weight of the unftamped pieces, before friction, was 13701,3 grs. Their total lofs, 199,3: the weight of the ftamped, 13679,5; the lofs 198,7.

All the pieces appeared to have fuffered more on the edges than on the faces; and thofe which were ftamped had the impreffion more or lefs obliterated or flattened, in proportion to their refpective degree of ductility, or to the lofs which according to the refult of this experiment, they had relatively fuftained. Edges worn moft,

The different pieces, after the experiment, had a curious appearance; for, on the edges, which were become round and polifhed, a fmall regular raifed bead or moulding was formed, which furrounded each face, like a frame; and both faces were become more or lefs concave. a raifed bead round the edge; the faces concave.

The original diameter of the pieces was alfo diminifhed, nearly according to their different degrees of ductility, and according to the lofs which they had experienced in confequence of the operation. The diameter diminifhed.

The meafure of the diameters of the pieces, after the experiment, was,

Gold 23 car. $3\frac{3}{4}$ grs. eight-tenths of an inch and $\frac{3}{40}$.

Gold alloyed with filver, nine-tenths of an inch.

The others varied little from nine-tenths and $\frac{1}{40}$; which was lefs, by about $\frac{1}{40}$ of an inch, than the original diameter of the pieces; and it was evident, that the pieces of fine gold, and thofe confifting of gold alloyed with filver, being the moft ductile, had fuffered the greateft lofs, and were moft diminifhed in diameter. Upon the whole, therefore, this experiment appears to corroborate what has been afferted concerning the former, *viz.* that foft or ductile gold fuffers the greateft lofs, when expofed to friction in contact with gold which is comparatively harder. Thefe experiments for afcertaining the effects arifing from the friction of coin againft coin being gone through, another feries was commenced with the apparatus, No. 3, by means of which various pieces were expofed to the action of certain powders and filings of metals, which were feparately fprinkled upon the horizontal table. Apparatus, No. 3.

The

The pieces were properly fixed in their refpective fockets and frames, and were placed fo as to bear upon the table, with or without additional weights.

The table was moved by a wheel and pinion, fo calculated as to avoid too rapid a motion; and the revolutions were denoted, as in the former experiments, by means of a counter.

Friction of gold by whiting, fand, filings of ftandard gold, and of iron.

The table was covered with fine powdered whiting, with fine white writing fand, with filings of gold made ftandard by copper, and laftly filings of iron. The laft three were fixed on the table by means of a folution of ifinglafs. Gold of different kinds as before, ftandard filver, and fine copper, both ftamped and unftamped, were fubjected to the different trials.

General refults.

From the whole of the preceding experiments, made with the three different machines, viewed and compared together, the author infers.

1ft. That when equal friction, affifted by a moderate preffure, takes place between pieces of coin of a fimilar quality, abrafion is moft commonly produced in an inverfe ratio to the ductility.

2d. That the contrary effect happens, when pieces of different qualities rub againft each other; for then, the more ductile metal is worn by that which is harder.*

3d. That earthy powders and metallic filings produce fimilar effects, and tend to wear the different kinds of gold in proportion to their refpective degrees of ductility.

Fine gold lofes its impreffion.

Fine gold, being extremely foft and ductile, fuftains a confiderable lofs, under many of the general circumftances of friction; and as at all times it appears certain, that the impreffions which have been ftamped upon it are moft eafily obliterated, even when actual abrafion does not take place, there is much reafon to conclude, that gold of fuch extreme ductility is not that which is the moft proper to be formed into coin.

Very hard gold improper for coin: Why.

But gold of the oppofite quality, or at leaft fo hard as to be juft capable of being rolled and ftamped, feems to be equally improper for the purpofe of coin. For, even fup-

* Some experiments made at Paris, in 1790, upon pure and upon alloyed filver are concifely mentioned, the refults of which appear to be nearly the fame as thofe of the prefent experiments upon gold.

pofing

pofing that hard gold fuffered, in every cafe, lefs by friction than that which is moderately ductile, (which is not however the fact,) and allowing that ftandard gold may, by a mixed alloy, be rendered as hard as gold reduced by copper to 18 carats, without changing the ftandard proportion of gold, yet it would be very difficult always to make fuch ftandard gold of an uniform degree of hardnefs. Moreover, by fome experiments which Mr. H. purpofely made at the Mint, upon the rolling and ftamping of gold of different qualities, it evidently appeared, that gold equal in hardnefs to that of 18 carats, could not be employed with advantage; for, the additional labour which was required for the rolling and ftamping of this hard gold, the frequent failure in making the impreffion, and the battering and breaking of the dies, fully proved, that the expence and difficulty attending the working of fuch gold, would by no means be compenfated by any fmall degree of durability which it might poffefs over any other.

The extremes of ductility and of hardnefs being therefore equally objectionable, it follows of courfe, that gold of moderate ductility muft be that which is the beft adapted for coin; and, as nothing but filver or copper can be employed to alloy gold which is intended to be coined, it may be here obferved, that whatever might have been the original motive for introducing the prefent ftandard of 22 carats, yet it appears, from the experiments lately defcribed, that this proportion of $\frac{1}{12}$ of the above-mentioned metals, is (every circumftance being confidered) the beft, or at leaft as good as any, which could have been chofen. Gold of moderate ductility beft.

There is, however, fome difference in the quality of gold, when alloyed with the ftandard proportion of filver, of filver and copper, and of copper, which requires to be confidered.

Gold alloyed with one-twelfth of filver, is of a fine but pale yellow; it is very ductile; it is eafily rolled, and may be ftamped without being annealed; it confequently does not require to be blanched; and, after the complete procefs of coining, the furface and every part remains of an uniform quality, fo that, by wear, it does not appear of different colours. Gold alloyed with filver, its advantages;

Thefe properties are certainly much to be valued; but the objections to this kind of ftandard gold are,

1ft.

Its difadvantages;

1ft. The additional expence attending the ufe of filver as an alloy.

2d. The extreme pale yellow colour. And,

3d. That, from its great ductility, it is almoft as liable to have the impreffions which have been made upon it obliterated, as thofe which have been made upon fine gold.

All things being therefore confidered, gold alloyed only with filver, does not appear to be fo proper for coin as may at firft be imagined.

with equal parts of filver and copper; its advantages;

Gold made ftandard by a mixture of equal parts of filver and copper, is not fo foft as gold alloyed only with filver; neither is it fo pale, for it appears to be lefs removed from the colour of fine gold than either the former or the following metal.

Gold alloyed with filver and copper, when annealed, does not become black, but brown; and this colour is more eafily removed by the blanching liquor, or folution of alum, than when the whole of the alloy confifts of copper. It may alfo be rolled and ftamped with great facility; and, under many circumftances, it appears to fuffer lefs by friction, than gold alloyed by filver, or by copper alone.

its difadvantages:

But, after it has been fubjected to the ordinary friction which muft take place during the circulation of money, it is liable to appear of a deeper colour in thofe parts which are prominent, and are confequently the moft expofed to friction. This defect arifes from a caufe which will foon be explained, but it cannot be regarded as an objection of any weight.

alloyed with copper alone.

The laft kind of ftandard gold which remains to be mentioned, is that which is alloyed only by copper. This is of a much deeper colour than thofe which have been hitherto noticed, and it is flightly harder than either of them; but neverthelefs it is very ductile, provided that the copper be pure. It requires to be annealed, and then becomes nearly or quite black: which colour is not fo eafily removed by the blanching liquor, as that which is produced by the procefs of annealing, upon gold alloyed with a mixture of filver and copper.

It fuffers lefs by many of the varieties of friction, than gold which is alloyed with filver; but, in fome cafes, it feems to wear rather more than gold alloyed with filver and copper; the difference is not however very confiderable.

This

This ſort of ſtandard gold, as well as that which is alloyed with ſilver and copper, appears commonly, after a certain degree of wear, of a coppery colour, more or leſs deep, in thoſe parts which are the moſt prominent; and, when coin thus alloyed exhibits ſuch an appearance, it is frequently and vulgarly ſaid to have been in contact with copper money; and ſometimes guineas having this appearance have been refuſed, upon the ſuppoſition that they were debaſed. But the real fact is, that when copper conſtitutes part or the whole of the alloy, it becomes oxidized or calcined upon the ſurface of the blanks, by the proceſs of annealing; and the blackiſh cruſt of copper, in this ſtate, muſt then be removed by the ſolution of alum, called the blanching liquor. Now it is evident, that after this operation, the ſurfaces of the blanks or unſtamped pieces, can no longer be regarded as ſtandard gold. For, if copper alone forms the alloy, it muſt be diſſolved and ſeparated from the ſurface of each piece of coin; and the ſame effect muſt alſo take place, with reſpect to the copper, in the alloy formed of copper and ſilver. So that, in the firſt caſe, each piece, when blanched, will conſiſt of gold made ſtandard by copper, covered with a thin coat of fine gold; and, in the ſecond caſe, each piece will be compoſed of gold made ſtandard by ſilver and copper, coated with gold alloyed with $\frac{1}{24}$ of ſilver, or with half of the ſtandard proportion of alloy, ſuppoſing the ſilver and copper to have been in equal quantities. As, therefore, the ſtandard gold of which the pieces conſiſt is always, more or leſs, of a deeper colour than the coating or film of the finer gold which covers each piece, it muſt be evident, that when this coating has been rubbed and removed from the raiſed or prominent parts, theſe will appear of a very different and deeper colour than the flat part or ground of the coin. The reaſon therefore is ſufficiently apparent, why gold which is alloyed with ſilver only, cannot be liable to this blemiſh.

Upon a compariſon of the different qualities of the three kinds of ſtandard gold which have been lately mentioned, it appears, (ſtrictly ſpeaking,) that gold made ſtandard by ſilver and copper is rather to be preferred for coin; but, as gold made ſtandard by copper alone is not very much inferior in its general properties, it may be queſtioned, whether the few advantages which are thus gained, will compenſate

Compariſon of the three kinds.

penſate the additional expence of the ſilver required for half of the alloy; and, indeed, any extraordinary addition of ſilver appears to be the leſs neceſſary, as there is commonly ſome ſilver in the gold which is ſent to the Mint, which, being reckoned as part of the alloy, contributes to produce thoſe beneficial effects which reſult when ſilver is purpoſely added.

From a general view of the preſent experiments, there does not appear to be any very great or remarkable difference in the comparative wear of the three kinds of ſtandard gold, all of which ſuffer abraſion ſlowly, and with much difficulty; and (as it has been already obſerved) the difference of wear between the two laſt mentioned, is certainly but inconſiderable. For theſe reaſons, and from the conſideration of every other circumſtance, it muſt be evident, that the extraordinary loſs which the gold coin of this kingdom is ſtated to have ſuſtained within a certain limited time, cannot, with even a ſhadow of probability, be attributed to any important defect in the compoſition or quality of the ſtandard gold; and all that can be ſaid upon this ſubject is, that ſome portion of this loſs may have been cauſed by the rough impreſſion and milled edge now in uſe, by which each piece of coin acts, and is acted upon by the others, in the manner of a file.

Extraordinary loſs ſuſtained by our gold coin not imputable to fair wear.

The loſs thus occaſioned cannot however be conſiderable; for the quality of the preſent ſtandard gold is certainly that which is well adapted to reſiſt abraſion, eſpecially in the caſe of the friction of coin againſt coin; and this is ſtrongly corroborated by the obſervations of bankers and others, who are in the babit of ſending or receiving large quantities of gold coin from any conſiderable diſtance. When a number of guineas, rather looſely packed, have been long ſhaken together by the motion of a coach or other carriage, the effects of friction are obſerved chiefly to fall upon only a few of the pieces, But it is not a little remarkable, that although theſe are often reduced nearly or quite to the ſtate of plain pieces of metal or blanks, yet, upon being weighed, they are found to have ſuſtained little or no loſs; and from this it appears, that the impreſſions have been obliterated, not by an actual abraſion of the metal, but by the depreſſion of the prominent parts, which have been forced into the maſs, and become reduced to a level with the ground of the coin. Pieces of hard gold would not ſo eaſily ſuffer by depreſ-

ſion;

fion; but the real lofs would probably be greater, they being, in the cafe of the friction of coin againft coin of fimilar quality, more fufceptible of abrafion.

Upon the whole, there is every reafon to believe, that our gold coin fuffers but little by friction againft itfelf; and the chief caufe of natural and fair wear probably arifes from extraneous and gritty particles, to the action of which the pieces may occafionally be expofed in the courfe of circulation. But ftill it muft be repeated, that the united effects of every fpecies of friction to which they may be fubjected, *fairly and unavoidably*, during circulation, cannot produce any other wear than that which is extremely gradual and flow, and fuch as will by no means account for the great and rapid diminution which has been obferved in the gold coin of this country.

SECTION II.

ON THE SPECIFIC GRAVITY OF GOLD WHEN ALLOYED BY VARIOUS METALS.

Difficulties in afcertaining the fpecific gravities of bodies, particularly from the inaccuracies of balances, the application and temperature of water, and the porofity of the object itfelf. Metals vary in their denfity by cafting in a mould, by fpeedy or flow cooling, and by hammering. The problem of finding fpecific gravities is attended with numerous difficulties.

Hammering and rolling is an imperfect remedy, and not applicable to the brittle metals. Hammering.

The effects of alloys on the fpecific gravity of gold are very intricate, and only capable of being determined by a direct trial: for a numerous feries of experiments clearly proved, not only that the fpecific gravity of the compound may differ from the mean of the component parts, but that the effect of the fame alloy, inftead of being proportionate to the quantity employed, may differ confiderably from this. To the peculiar effects produced by certain proportions of fome of the metals muft be added the effects peculiar to certain compound alloys, whence arifes an immenfe complicated feries of alterations in fpecific gravity, never yet inveftigated by philofophers. Alloys affect the fpecific gravity of gold very fingularly. Compound alloys.

With regard to the expanfion or contraction of the compound, little alteration appears to be produced by alloying gold with $\frac{1}{12}$ of pure filver, as it produced only an expanfion of 0,10. With copper it was 0,66: with equal parts of filver and copper Expanfion produced in gold by filver; copper;

Iron. per the expanfion was 0,67; with iron it was 0,44; with iron and copper 0,37. Tin, bifmuth, zinc, and antimony, produced a contraction. Lead and bifmuth very much refemble each other in their effects on gold, and in the irregularity of thefe in various proportions.

Contraction by tin, bifmuth, zinc, and antimony.

Miftake of Briffon in the alloy of gold with copper.

Mr. Briffon has obferved, that on alloying gold with $\frac{1}{12}$ of copper, a mutual penetration takes place; but in the courfe of the prefent experiments the reverfe of this was found. It is probable, therefore, that his experiment was made with part of a large bar or ingot; as the unequal diffufion of the alloy, the quantity of the metal, and the nature, form and pofition of the mould, are all capable of affecting the fpecific gravity. Thus, when the mixture is perfect, the bottom of a bar caft in a vertical mould will be of the greateft fpecific gravity, owing to the preffure of the fuperincumbent metal, while the quality of both ends appears equal by the affay. On the contrary, if the mixture be imperfect, the lighter metal flowing out of the crucible firft, will render the bottom of the bar inferior both in quality and in fpecific gravity, as was found by experiment.

Specific gravity of a mixed metal affected by various circumftances. Differences in the fame bar. Whence.

Unequal diffufion of alloy.

This unequal diffufion of the alloy through the mafs, the exact diftribution of which is not fo eafy as may be imagined, particularly in large quantities, is the moft frequent caufe of the variation in the fpecific gravity of ftandard gold. The difficulty has been confidered, and an allowance made for it, called the remedy for the mafter of the mint. Even when metals have been completely mixed, if they be kept in fufion under certain circumftances, a feparation, more or lefs perfect, fometimes takes place. This feparation appears to be according to the relative affinities and fpecific gravities of the two metals, and is the fooneft effected when the metals have not been perfectly mixed *.

Mixed metals feparate under fufion.

Long continued friction leffens fpecific gravity of metals.

Befide the caufes mentioned there is another, that occafions variations more or lefs confiderable in the fpecific gravity of metals,

True chemical combinations.

* Some compound metals may perhaps be mere mechanical mixtures; but I am inclined to believe, that by much the greateft number are true chemical combinations; and confequently, when thefe laft have been properly formed, a feparation of the component metals, by the means above-mentioned, can feldom if ever be effected. *C. H.*

metals, and does not appear to have been noticed. This is long continued friction, which always produced a diminution in the specific gravity of the pieces of metal exposed to it.

Among the other less powerful causes which produce some alteration in the specific gravity of gold, the processes of rolling, and of annealing, may also be enumerated; for, in the course of these experiments it appeared that the specific gravity of the bars, &c. was in a small degree increased by rolling, and that the contrary effect was produced by annealing.

Rolling increases it:

Annealing diminishes it.

The specific gravity of gold, 23 car. $3\frac{1}{4}$ grs. fine, when rolled and stamped without being annealed, was found to be 19,277; but, when the same was annealed, the specific gravity was 19,231, after stamping.

Mr. H. is however, inclined to believe, that annealing had reduced the specific gravity to much less than is here stated; and that the subsequent operation of stamping had, in some measure, compensated the effects of annealing. For, in the experiments lately mentioned, it was proved, that the specific gravity of the pieces which had not been annealed, was reduced, by long continued friction, from 19,277 to 19,171; an effect surpassing that which resulted from annealing by ,060 (19,231 − 19,171 = ,060;) and, if heat was the cause, the reverse might have been expected, inasmuch as the annealing heat exceeded that which was produced by friction; but, as this was not the case, he is induced to be of opinion, that the specific gravity was again increased, by the subsequent stamping of the annealed pieces.

In addition, therefore, to those causes of variation in specific gravity which are the immediate consequences of hydrostatical operations, such as, the different height of the column of water, and the changes of temperature to which it is exposed during the experiments, the following, as far as they concern metallic substances, may be enumerated.

Causes of variation in specific gravity enumerated.

1. Imperfections in the interior of the mass, which are produced during the processes of melting and casting.

2. The difference of density in parts of the same mass, resulting from the quality and quantity of the metal, from the nature of the mould, from the more or less vertical position of it, and from the height of the column or bar of metal which is cast.

3. The

3. The unequal diſtribution of the metal, or metals, employed as an alloy, throughout the maſs intended to be alloyed.

4. The peculiar effects which certain metals produce, when uſed ſingly or conjointly as alloys, and which are very different from the reſults of calculation.*

5. Heat, whether produced by friction or excited in any other manner.

Abſolute preciſion not to be expected.

As, therefore, the ſpecific gravity of metals is liable to be influenced by ſuch a variety of cauſes, it is almoſt in vain to expect abſolute preciſion in the reſults of experiments made by different perſons; but, at the ſame time, it may be obſerved, that by proper care and attention to the above circumſtances, a degree of accuracy may be attained, ſufficient to anſwer almoſt every uſeful purpoſe, although, from what has been ſaid, it muſt appear improper to form opinions upon ſmall fractional variations. By the experiments which were made, with every poſſible precaution, upon ſeparate and intire ingots of gold, reduced to ſtandard by ſilver, by ſilver and copper, and by copper alone, when caſt in an iron mould like a cupel, it appeared, that the ſpecific gravity of each of theſe kinds of ſtandard gold is as follows.

Specific gravity of ſtandard gold of different kinds.

Gold made ſtandard by ſilver - - - -	17,927
Gold made ſtandard by ſilver and copper -	17,344
Gold made ſtandard by copper - - - -	17,157

That of our ſtandard gold muſt occaſionally differ.

Now, as our gold coin commonly contains ſilver as part of the alloy, and as at different times this proportion of ſilver muſt have been various, and even conſiderable, particularly when the gold of Portugal, which is alloyed with ſilver, was brought to the Mint, it naturally follows that, excluſive of the many other cauſes of variation which have lately been enumerated, the ſpecific gravity of our ſtandard gold muſt occaſionally be different,

* There can be no doubt but that the effects of compound alloys are, in general, very different from thoſe of each metal ſeparately conſidered; and that ſuch metallic combinations or compound alloys, like neutral ſalts, and many other compounds, have peculiar properties, which act variouſly upon the metals to which theſe compound alloys are added. A great number of accurate experiments are, however, requiſite to elucidate a queſtion ſo intricate.

It may here be alſo obſerved, that the peculiar properties of compound alloys, prove them to be real chemical combinations. *C. H.*

according

according to the relative proportions of ſilver and copper which compoſe the alloy;* and, as the ſpecific gravity of gold made ſtandard by ſilver is, in the ingot caſt under the above circumſtances, 17,927, while that of gold made ſtandard by copper is only 17,157, ſo, according to the relative proportions of theſe two metals, when united in the alloy, the ſpecific gravity of the ſtandard gold may vary between the two extremes of 17,927 and 17,157, although the real quality or value of the ſtandard gold remains unchanged; and indeed, when ſome allowance is alſo made for ſmall variations ariſing from other cauſes, the range of the different ſpecific gravities of gold made ſtandard by ſilver and copper, may be conſidered as nearly extending from 18 to 17.

Specific gravity of fine gold eſtimated too high. Miſtaken notions injurious to our coin. Our old coin not ſo good as the new.

There is much reaſon to believe, that the ſpecific gravity of fine gold has been too highly eſtimated; and hence a notion has been too commonly received in this country, which has injuriouſly and unjuſtly been believed on the continent, that the ſtandard gold of the preſent reign is inferior to that of the reigns preceding it. But the real fact is preciſely the reverſe. If a few of the old coins have proved better than ſtandard, they were much inferior in the aggregate.

II.

A Memoir on the Appearance of Spectres or Phantoms occaſioned by Diſeaſe, with Pſychological Remarks. Read by NICOLAI *to the Royal Society of Berlin, on the 28th of February,* 1799.

On the philoſophical diviſions reſpecting body and mind.

PHILOSOPHERS divide the human being into body and mind, becauſe the numerous and diſtinct obſervations we make on ourſelves oblige us to conſider man particularly, as well in reſpect to his corporeal as his mental functions. Other philoſophers have ſuppoſed that this ſubject might be treated with greater perſpicuity by conſidering man as compoſed of body, ſoul, and mind. There can be no doubt but that theſe, and even more diviſions might be invented. Such philoſophers,

* The firſt guineas which were coined, or thoſe of CHARLES II. and JAMES II. were generally alloyed with ſtandard ſilver; but the coins of the ſubſequent reigns have been alloyed with copper, added to compenſate the deficiency of alloy, or of ſilver in the gold.

however, have by no means confidered that arbitrary fyftematic divifions, do not conftitute an inveftigation of nature, and that philofophy often becomes more uncertain the more precifely we endeavour to diftinguifh and feparate what nature has clofely united. Sub-divifions in fpeculation feem as neceffary as fences in fields, both are in themfelves unproductive, and the more they are multiplied and extended the greater is the diminution of the fertility.

They are of little ufe,

and quite inaccurate.

For my part, I will confefs, that I do not know where the corporeal effence in man ceafes, or where the mental begins; though I admit of the diftinction, becaufe the extreme differences can be clearly perceived. If we divide man into three parts, we fhall be far from removing the difficulties, as we fhould be were we even to follow thofe modern philofophers, who regard the thinking fubject alone as the real Being (*Ego*,) and confider all external appearances as confined to the ideas of confcious beings. The greateft and moft peculiar difficulties in the philofophic knowledge of the human fubject confifts in this, that we have never yet been able clearly and diftinctly to afcertain the internal affociation of thofe ftriking differences which we obferve in our being. Neither the moft fubtil phyfiology nor the fineft fpeculative philofophy, have yet been able to explain the union of thought and phyfical operations. We may indeed doubt whether the labours of our German philofophers, though founded jointly upon modern fpeculation and modern chemiftry, will be attended with any greater fuccefs. Extreme caution is moft undoubtedly requifite to prevent our becoming too intimately and habitually acquainted with certain hypothetical notions refpecting things really unknown, fo as to miftake them for truths and deduce erroneous conclufions.

Speculations on the nature of thought are delufive.

It is much to be feared that the hypothefes and poftulates of fpeculation will be of little value in this cafe; though to us they may feem very confiftent and clear, while we regard them only in a certain point of view. An attention to experimental proof may bring us nearer to our aim, though its perfect accomplifhment will perhaps never be within the reach of human inveftigation. Experiments or facts may fhew the corporeal as well as the mental functions in feveral lights, and in fuch as we never can perceive by mere fpeculation.

Advantages of refearch.

Though it is truly faid that the firft principles of nature are placed beyond our reach, yet an endeavour to penetrate into the

the interior of nature will always prove beneficial to the human mind; as long as we do not presume to have completely investigated the subject; but continue our exertions by uniting the observations of facts with deliberate reasoning.

The hypothesis of body and mind has led to many disquisitions respecting ghosts or spirits.

Since men have forgotten that what philosophy has separated is not on that account separated in nature, and since from the earliest ages, the mind and body of man have been considered as if distinct from each other, numberless questions have arisen which have given room for much controversy, without having met with any satisfactory answer. For example: Whether after the dissolution of the body, the spirit (or mind) continues to exist without the body? Whether the spirit can act without the body, and in what manner? And lastly, it is also a question, Whether, as we consider a disembodied spirit not only in a state of separate continual existence, but also in a state of continual existence and continual action *amongst us*, a mere spirit and its actions cannot become perceptible to our senses?—Whether the figure of a spirit (and in particular that of a deceased person) may not be seen? and, Whether a sound proceeding from it may not affect the ear of the living? All the knowledge usually considered as possible to be had of a departed spirit is confined to seeing and hearing; for as far as my information extends, the devil is the only spirit that enjoys the privilege of affecting the sense of smell at his departure.

Why the narratives of apparitions are generally concluded to be imposture.

We have less motive for disputing about the absolute possibility of seeing a spirit, because the idea of a spirit is so indistinct and vague, and because the words spirit and body in considering man, do in reality indicate mere relative notions. It is inconsistent with every known law of nature to suppose that those terms of relation adopted by us solely for the purpose of investigating the nature of man do themselves possess any separate and independent existence. This argument causes a suspicion of deceit or imposition always to attach to narratives of the apparitions of disembodied spirits. But those who are inclined to see and hear spirits, are not satisfied with this summary solution; they appeal to experience, against which no maxim *a priori* can hold. This only is required, that the experience must be true and well attested.

But the delusions of imagination or disease well deserve to be investigated.

Individuals who pretend to have seen and heard spirits are not to be persuaded that their apparitions were simply the creatures of their senses. You may tell them of the impositions that

that are frequently practiſed, and the fallacy which may lead us to take a ſpirit of our imagination by moon-light for a corpſe. We are generally adviſed to ſeize the ghoſts, in which caſe it is often found that they are of a very corporeal nature.—An appeal is alſo made to ſelf-deception, becauſe many perſons believe they actually ſee and hear where nothing is either to be ſeen or heard. No reaſonable man, I think, will ever deny the poſſibility of our being ſometimes deceived in this manner by our fancy, if he is in any degree acquainted with the nature of its operations. Nevertheleſs, the lovers of the marvellous will give no credit to theſe objections, whenever they are diſpoſed to conſider the phantoms of imagination as realities. We cannot therefore ſufficiently collect and authenticate ſuch proofs as ſhew how eaſily we are miſled; and with what deluſive facility the imagination can exhibit, not only to deranged perſons, but alſo to thoſe who are in the perfect uſe of their ſenſes, ſuch forms as are ſcarcely to be diſtinguiſhed from real objects.

Striking inſtances of apparitions ſeen by the author.

I myſelf have experienced an inſtance of this, which not only in a pſychological, but alſo in a medical point of view appears to me of the utmoſt importance. I ſaw, in the full uſe of my ſenſes, and (after I had got the better of the fright which at firſt ſeized me, and the diſagreeable ſenſation which it cauſed) even in the greateſt compoſure of mind, for almoſt two months conſtantly, and involuntarily, a number of human and other apparitions;—nay, I even heard their voices;—yet after all, this was nothing but the conſequence of nervous debility, or irritation, or ſome unuſual ſtate of the animal ſyſtem.

The publication of the caſe in the Journal of Practical Medicine, by Profeſſor Hufeland of Jena, is the cauſe of my now communicating it to the Academy. When I had the pleaſure of ſpending a few happy days with that gentleman laſt ſummer, at Pyrmont, I related to him this curious incident.

Narrative and remarks on ſpectres and apparitions.

But as it is probable he might not diſtinctly remember that which I had told altogether accidentally, perhaps indeed not very circumſtantially, ſome conſiderable errors have been admitted into his narrative. In ſuch a caſe, however, it is more neceſſary than in any other, to obſerve every thing with accuracy, and to relate it with fidelity and diſtinctneſs. I ſhall therefore paſs over nothing which I remember with any degree of certainty. Several incidents connected with the apparitions

paritions feem to me of great importance; though we might be apt to regard them in a fecondary point of view; for we cannot determine of what confequence even a circumftance of the moft trivial nature may be, if at any future period (in cafe more experiments of a like nature are afcertained) fome fuppofitions or conclufions can be made refpecting the origin of fuch phantoms, or on fome law of the affociation of ideas according to which they are modified or follow one another.

Narrative and remarks on fpectres produced by nervous indifpofition.

I was alfo, which is feldom the cafe, in a fituation to make obfervations on myfelf. I took down therefore in a few words what was moft important, and recounted it immediately to feveral perfons. My memory, which is extremely retentive, has befides treafured up the moft minute circumftances; the more on that account, as this ftory has very often proved the fubject of my impartial confideration, not only with regard to my own particular fituation but alfo in refpect to its many pfychological confequences. Its truth will, I hope, require no further affurance on my part, fince a member of this academy (Mr. Selle) is an unexceptionable witnefs of it, having, as my phyfician, received a daily account of all that happened to me.

It would be extremely improper in an affembly like the prefent to fpeak much of myfelf; it can only be excufable in this particular cafe, where it ferves to throw greater light on fcientific inveftigation. I muft requeft permiffion therefore to notice feveral particulars of my fituation previous to my feeing the phantoms, as thofe incidents may have greatly affected the ftate of my body and mind during that time.

In the laft ten months of the year 1790, I underwent feveral very fevere trials, which greatly agitated me. From the month of September in particular, repeated fhocks of misfortune had befallen me, which produced the deepeft forrow. It had been ufual for me to lofe blood by venefection twice a year. This was done once on the 9th of July 1790, but towards the clofe of the year it was omitted. In 1783 I had been fuddenly feized with a violent giddinefs, which the phyfician imputed to an obftruction in the fmall mufcles of the *abdomen*, proceeding from too intenfe an application to ftudy, and my fedentary manner of life for many years. Thefe complaints were removed by a three years cure, and the rigid obfervance of a ftrict diet during that time. In the firft ftage of the malady the application of leeches to the *anus* had been particularly ef-

fective,

Narrative and remarks on ſpectres produced by nervous indiſpoſition.

fective, and this remedy I had from that time regularly applied twice or thrice a year, whenever I felt congeſtion in the head. It was on the 1ſt of March 1790 that the leeches had been laſt applied; the bleeding therefore and the clearing of the minuter blood-veſſels by leeches had, in 1790 been leſs frequently obſerved than uſual. A circumſtance too that could not tend to benefit my deplorable ſituation was, that from September I had been continually engaged in buſineſs which required the ſevereſt exertion, and which, from frequent interruptions, was rendered ſtill more burthenſome and diſtreſſing.

In the firſt two months of the year 1791, I was much affected in my mind by ſeveral incidents of a very diſagreeable nature; and on the 24th of February a circumſtance occurred which irritated me extremely. At ten o'clock in the forenoon my wife and another perſon came to conſole me; I was in a violent perturbation of mind, owing to a ſeries of incidents which had altogether wounded my moral feelings, and from which I ſaw no poſſibility of relief; when ſuddenly I obſerved at the diſtance of ten paces from me a figure,—the figure of a deceaſed perſon. I pointed at it, and aſked my wife whether ſhe did not ſee it. She ſaw nothing, but being much alarmed, endeavoured to compoſe me, and ſent for the phyſician. The figure remained ſome ſeven or eight minutes, and at length I became a little more calm; and as I was extremely exhauſted, I ſoon afterwards fell into a troubled kind of ſlumber, which laſted for half an hour. The viſion was aſcribed to the great agitation of mind in which I had been, and it was ſuppoſed I ſhould have nothing more to apprehend from that cauſe; but the violent affection had put my nerves into ſome unnatural ſtate, from this aroſe further conſequences, which require a more detailed deſcription.

In the afternoon, a little after four o'clock, the figure which I had ſeen in the morning again appeared. I was alone when this happened; a circumſtance which, as may be eaſily conceived, could not be very agreeable. I went therefore to the apartment of my wife, to whom I related it. But thither alſo the figure purſued me. Sometimes it was preſent, ſometimes it vaniſhed, but it was always the ſame ſtanding figure. A little after ſix o'clock ſeveral ſtalking figures alſo appeared; but they had no connection with the ſtanding figure. I can aſſign no other reaſon for this apparition than that, though much more compoſed

compofed in my mind, I had not been able fo foon entirely to forget the caufe of fuch deep and diftreffing vexation, and had reflected on the confequences of it, in order, if poffible, to avoid them; and that this happened three hours after dinner, at the time when the digeftion juft begins.

Narrative and remarks on fpectres produced by nervous imagination.

At length I became more compofed with refpect to the difagreeable incident which had given rife to the firft apparition; but though I had ufed very excellent medicines, and found myfelf in other refpects perfectly well, yet the apparitions did not diminifh, but on the contrary rather encreafed in number, and were transformed in the moft extraordinary manner.

After I had recovered from the firft impreffion of terror, I never felt myfelf particularly agitated by thefe apparitions, as I confidered them to be what they really were, the extraordinary confequences of indifpofition; on the contrary, I endeavoured as much as poffible to preferve my compofure of mind, that I might remain diftinctly confcious of what paffed within me. I obferved thefe phantoms with great accuracy, and very often reflected on my previous thoughts, with a view to difcover fome law in the affociation of ideas, by which exactly thefe or other figures might prefent themfelves to the imagination.—Sometimes I thought I had made a difcovery, efpecially in the latter period of my vifions; but on the whole I could trace no connexion which the varions figures that thus appeared and difappeared to my fight had, either with my ftate of mind, or with my employment, and the other thoughts which engaged my attention. After frequent accurate obfervations on the fubject, having fairly proved and maturely confidered it, I could form no other conclufion on the caufe and confequence of fuch apparitions than that, when the nervous fyftem is weak and at the fame time too much excited, or rather deranged, fimilar figures may appear in fuch a manner as if they were actually feen and heard; for thefe vifions in my cafe were not the confequence of any known law of reafon, of the imagination, or of the otherwife ufual affociation of ideas; and fuch alfo is the cafe with other men, as far as we can reafon from the few examples we know.

The origin of the individual pictures which prefent themfelves to us, muft undoubtedly be fought for in the ftructure of that organization by which we think; but this will always.

Narrative and remarks on ſpectres produced by nervous indiſpoſition.

always remain no leſs inexplicable to us than the origin of thoſe powers by which conſciouſneſs and fancy are made to exiſt.

The figure of the deceaſed perſon never appeared to me after the firſt dreadful day; but ſeveral other figures ſhewed themſelves afterwards very diſtinctly; ſometimes ſuch as I knew, moſtly, however, of perſons I did not know, and amongſt thoſe known to me, were the ſemblances of both living and deceaſed perſons, but moſtly the former: and I made the obſervation that acquaintance with whom I daily converſed never appeared to me as phantaſms; it was always ſuch as were at a diſtance. When theſe apparitions had continued ſome weeks, and I could regard them with the greateſt compoſure, I afterwards endeavoured, at my own pleaſure to call forth phantoms of ſeveral acquaintance, whom I for that reaſon repreſented to my imagination in the moſt lively manner, but in vain.—For however accurately I pictured to my mind the figures of ſuch perſons, I never once could ſucceed in my deſire of ſeeing them *externally;* though I had ſome ſhort time before ſeen them as phantoms, and they had perhaps afterwards unexpectedly preſented themſelves to me in the ſame manner. The phantaſms appeared to me in every caſe involuntarily, as if they had been preſented externally, like the phenomena in nature, though they certainly had their origin internally; and at the ſame time I was always able to diſtinguiſh with the greateſt preciſion phantaſms from phenomena. Indeed, I never once erred in this, as I was in general perfectly calm and ſelf-collected on the occaſion. I knew extremely well, when it only appeared to me that the door was opened, and a phantom entered, and when the door really was opened and any perſon came in.

It is alſo to be noted, that theſe figures appeared to me at all times, and under the moſt different circumſtances, equally diſtinct and clear. Whether I was alone, or in company, by broad day-light equally as in the night time, in my own as well as in my neighbour's houſe; yet when I was at another perſon's houſe, they were leſs frequent, and when I walked the public ſtreet they very ſeldom appeared. When I ſhut my eyes ſometimes the figures diſappeared, ſometimes they remained even after I had cloſed them. If they vaniſhed in the former caſe, on opening my eyes again, nearly the ſame figures appeared which I had ſeen before.

I ſome-

Narrative and remarks on ſpectres produced by nervous indiſpoſition.

I ſometimes converſed with my phyſician and my wife, concerning the phantaſms which at the time hovered around me: for in general the forms appeared oftener in motion than at reſt. They did not always continue preſent—they frequently left me altogether, and again appeared for a ſhort or longer ſpace of time, ſingly or more at once; but, in general, ſeveral appeared together. For the moſt part I ſaw human figures of both ſexes; they commonly paſſed to and fro as if they had no connection with each other, like people at a fair where all is buſtle; ſometimes they appeared to have buſineſs with one another. Once or twice I ſaw amongſt them perſons on horſeback, and dogs and birds; theſe figures all appeared to me in their natural ſize, as diſtinctly as if they had exiſted in real life, with the ſeveral tints on the uncovered parts of the body, and with all the different kinds and colours of clothes. But I think, however, that the colours were ſomewhat *paler* than they are in nature.

None of the figures had any diſtinguiſhing characteriſtick, they were neither terrible, ludicrous, nor repulſive; moſt of them were ordinary in their appearance,—ſome were even agreeable.

On the whole, the longer I continued in this ſtate, the more did the number of phantaſms encreaſe, and the apparitions became more frequent. About four weeks afterwards I began to hear them ſpeak: ſometimes the phantaſms ſpoke with one another; but for the moſt part they addreſſed themſelves to me: theſe ſpeeches were in general ſhort, and never contained any thing diſagreeable. Intelligent and reſpected friends often appeared to me, who endeavoured to conſole me in my grief, which ſtill left deep traces on my mind. This ſpeaking I heard moſt frequently when I was alone: though I ſometimes heard it in company, intermixed with the converſation of real perſons; frequently in ſingle phraſes only, but ſometimes even in connected diſcourſe.

Though at this time I enjoyed rather a good ſtate of health both in body and mind, and had become ſo very familiar with theſe phantaſms, that at laſt they did not excite the leaſt diſagreeable emotion, but on the contrary afforded me frequent ſubjects for amuſement and mirth; yet as the diſorder ſenſibly encreaſed, and the figures appeared to me for whole days together, and even during the night, if I happened to awake, I had

Narrative and remarks on fpectres produced by nervous indifpofition.

I had recourfe to feveral medicines, and was at laft again obliged to have recourfe to the application of leeches to the anus.

This was performed on the 20th of April at eleven o'clock in the forenoon. I was alone with the furgeon, but during the operation, the room fwarmed with human forms of every defcription, which crouded faft one on another; this continued till half paft four o'clock, exactly the time when the digeftion commences. I then obferved that the figures began to move more flowly; foon afterwards the colours became gradually paler; every feven minutes they loft more and more of their intenfity, without any alteration in the diftinct figure of the apparitions. At about half paft fix o'clock all the figures were entirely white, and moved very little; yet the forms appeared perfectly diftinct; by degrees they became vifibly lefs plain, without decreafing in number, as had often formerly been the cafe. The figures did not move off, neither did they vanifh which alfo had ufually happened on other occafions. In this inftance they diffolved immediately into air; of fome even whole pieces remained for a length of time, which alfo by degrees were loft to the eye. At about eight o'clock there did not remain a veftige of any of them, and I have never fince experienced any appearance of the fame kind. Twice or thrice fince that time I have felt a propenfity, if I may be fo allowed to exprefs myfelf, or a fenfation as if I faw fomething which in a moment again was gone. I was even furprifed by this fenfation whilft writing the prefent account, having, in order to render it more accurate, perufed the papers of 1791, and recalled to my memory all the circumftances of that time. So little are we fometimes, even in the greateft compofure of mind, mafters of our imagination.

This is an exact narrative of the apparitions which I obferved during the difordered ftate of my nerves: and I fhall now add a few obfervations, partly with the intention of explaining their origin from other obfervations made on myfelf, and partly with a view of pointing out at leaft fome diftant pfychological confequences, which might be deduced from this remarkable cafe.

Experience fhews that we may, in various manners, imagine that we fee figures, and even hear them when they do not really exift.

1ft

Narrative and remarks on fpectres produced by nervous indifpofition.

1ft, And commonly this may happen in dreams.—The manner of dreaming is different in every individual, and probably depends on the joint effects of the powers of intellect, and thofe by which the impreffions of the fenfes are received, and thefe are modified by the ftate of the fyftem at each particular time. I have myfelf made fome remarkable obfervations on the nature of my dreams, and compared them with fome obfervations on that fubject which have been communicated to me by others.

2d. In every degree of mental derangement till abfolute infanity.

3d. In fevers of the brain, which for a fhort time, or at certain intermitting periods, occafion a delirium.

4th. By the mere power of imagination without any fever, when in other refpects the judgment is perfectly found. In this cafe it is very difficult to difcover the truth, unlefs we combine an accurate habit of obfervation with the moft impartial fcrutiny.

Inftances are too frequent in which we are impofed upon, not by the imagination, but by delufion of the judgment. How many are there, who prefer the marvellous and affume an air of importance, when they have an opportunity of relating wonderful things of themfelves—How few are there who endeavour to diveft themfelves of prejudice, or to check their imagination; and ftill fewer are they who are accurate in their obfervations, efpecially in fuch as relate to themfelves; even thofe who have fufficient firmnefs to adhere ftrictly to the truth form an inconfiderable number. Hence it is, that when a perfon relates any ftrange incident, he either detracts or magnifies, and will even fancy that he has verified fome facts, which he has invented only at the moment that he relates them. This laft is the cafe with a clafs of men who obftinately perfift in their own opinions, and frequently affert more than they can fupport, merely with a view to maintain what they have once advanced. All the above mentioned circumftances feem to have coincided in the celebrated vifions of Emanuel Swedenborg. He delighted in fpeculation and myftical theology; he had formed a fyftem for himfelf in which ghofts were neceffary, and it was his primary view to eftablifh this wonderful fyftem. It is poffible that he may have feen phantafms, the more fo as he

ftudied

Narrative and remarks on spectres produced by nervous indisposition.

studied much, and was a great eater.* But in order to appear a prodigy to the world, he embellished his visions on which he wrote voluminous treatises, by creating new images in conformity with his own system.

Lastly, those who are most conversant with the marvellous, give but a very indistinct idea of their visions. This I have found in conversation with persons who in other respects were very worthy characters, but who were great admirers of the what are termed occult sciences, which they cultivated to such a degree, that to give you a notion of it here would seem preposterous. I have frequently discoursed on spirits, and the seeing of spirits, with a person who ranked very high in the school of secret wisdom, but who was otherwise a man of a very limited capacity, and rather ignorant in all those sciences which enlighten the mind. This person told me amongst other things that he should feel very unhappy, were he not continually in company with spirits. As I have always taken a pleasure in the clear developement of human opinions, however absurd they may appear, I was desirous to learn in what manner he saw the spirits, and how he came into company with them?—But here he would not allow of the appearance of any corporeal forms; he assured me that spirits were only to be seen with the eyes of the spirit: then he added in a very serious tone, "Just as the human soul is *Naephaesch*, "or a branch taken off the tree, so are all spirits branched off "from the supreme spirit, as it in the astringent motion com-"pressed its being." On nearer enquiry I could easily perceive that he entertained a confused notion of the cabalistick ontology of Spinoza, and that he imagined all the powers in nature to be spirits. What he meant to say therefore, was neither more nor less than that he should feel unhappy, did he live in a world where nature was perfectly inanimate; if he could not think that every thing around him was in the continual and mutual *exercise* of its powers. In this belief then he peopled all space with spirits, nearly in the same manner as the antient mythology peopled the woods with Dryads and

* On this subject, the review of Swedenborg's Works in the *Allgemeine Deutsche Bibliothek*, vol. 107, p. 15, is very interesting. In it the resemblance of Swedenborg's system, with the visions of the German enthusiast, Johann Tennhart, is clearly accounted for: he also was a great eater.

Hama-

Narrative and remarks on spectres produced by nervous indisposition.

Hamadryads. Indeed, every thing properly confidered, the opinion of my cabalift is not quite fo very abfurd as you may fuppofe; for in reality, the word *power* is with the philofopher only that which the × is to the mathematician; and, if I be not altogether miftaken, the mathematician can with his ×, bring more clear truths to light, than the philofopher by the word *power*. If a given power cannot be rendered fubfervient to deduction, fo that, like Newton's *calculus*, it fhall perfectly accord with experience; nothing more will be determined or explained by the mere word power, than by the word fpirit; and I doubt much whether the new judicious Kantian fyftem of Dynamic natural philofophy, which confiders all bodies as mere aggregates of powers, would not rather cut the gordian knot than unravel it.

It is not very uncommon that by a derangement of the corporeal powers, even without infanity and inflammatory fevers, apparitions do ftrike the eye externally, which are only internally the production of the imagination. The experience of this may teach us a leffon of forbearance, not rafhly to confider as impoftors thofe well difpofed perfons who believe they have feen apparitions. But as manifold experience fhews us how far the human imagination can go in the external reprefentation of pictures; it may alfo admonifh thofe well-difpofed perfons not to afcribe to their vifions any degree of reality, and ftill lefs to confider the effects of a difordered fyftem, as proofs that they are haunted by fpirits.

The celebrated Juftus Mofer frequently believed that he faw flowers. Another of my acquaintance fees in like manner, at times, mathematical figures, circles, fquares, &c. in different colours. More examples of this kind may perhaps be found in Moretz's Magazine, in Krueger's Experimental Pfychology, and in Bonnet's Pfychological writings. The hearing of founds is a cafe which feldomer occurs. My much-lamented friend Mofes Meudeljohn had, in the year 1792, by too intenfe an application to ftudy, contracted a malady, which alfo abounded with particular pfychological apparitions. For upwards of two years he was incapacitated from doing any thing; he could neither read nor think, and was rendered utterly incapable of fupporting any loud noife. If any one talked to him rather in a lively manner, or if he himfelf happened to be difpofed to lively converfation, he

fell

Narrative and remarks on ſpectres produced by nervous indiſpoſition.

fell in the evening into a very alarming ſpecies of catalepſis, in which he ſaw and heard every thing that paſſed around him, without being able to move a limb. If he had heard any lively converſation during the day, a ſtentorian voice repeated to him while in the fit, the particular words or ſyllables that had been pronounced with an impreſſive accent, or loud emphatic tone, and in ſuch a manner that his ears reverberate.

Seldom as it may happen, that perſons believe they ſee human forms, yet examples of the caſe are not wanting. A reſpectable member of this academy, diſtinguiſhed by his merit in the ſcience of botany, whoſe truth and credibility are unexceptionable, once ſaw in this very room in which we are now aſſembled, the phantaſm of the late preſident Maupertuis. A perſon of a ſound and unprejudiced mind, though not a man of letters, whom I know well, and whoſe word may be credited, related to me the following caſe. As he was recovering from a violent nervous fever, being ſtill very weak, he lay one night in bed perfectly conſcious that he was awake, when the door ſeemed to open, and the figure of a woman entered, who advanced to his bed-ſide. He looked at it for ſome moments, but as the ſight was diſagreeable, he turned himſelf and awakened his wife; on turning again however he found the figure was gone. But out of many caſes I have never known an inſtance like my own, in which any perſon had for almoſt two months conſtantly beheld ſuch viſionary forms, and ſeemed even to have heard them; except it was that of two young ladies, who, as I have been credibly informed, frequently ſaw appearances of this nature.

I am by no means inſenſible to a certain feeling which admoniſhes me of the impropriety of talking ſo much of myſelf in an aſſembly like this; but ſince I tranſgreſs only with a ſcientific intention, to contribute to the knowledge of the effects of the human imagination, I muſt endeavour to ſuppreſs this feeling. I may look for pardon, I truſt, from thoſe who know and reſpect every thing which tends to enlarge the ſtock of human knowledge, even if I ſpeak more of myſelf. For, when I proceed to deſcribe the ſtate of my imagination, and the nature of the apparitions during a previous malady, it will be merely with an intention to ſhew the apparitions which form the ſubject of this lecture in a leſs wonderful point of view,

and

and by that means perhaps to contribute in ſome degree to the illuſtration of ſo ſtrange an incident.

Narrative and remarks on ſpectres produced by nervous indiſpoſition.

I muſt obſerve that my imagination poſſeſſes in general a great facility in picturing. I have for example ſketched in my mind a number of plans for novels and plays; though I have committed very few of them to paper, becauſe I was leſs ſolicitous to execute than to invent. I have generally arranged theſe outlines when, in a chearful ſtate of mind. I have taken a ſolitary walk, or when travelling I have ſat in my carriage, and could only find employment in myſelf and my imagination. Conſtantly and even now do the different perſons whom I imagine in the formation of ſuch a plot, preſent themſelves to me in the moſt lively and diſtinct manner: their figure, their features, their manner, their dreſs, and their complexion, are all viſible to my fancy. As long as I meditate on a fixed plan, and afterwards carry it into effect,—even when I am often interrupted, and muſt begin it again at different times, all the acting perſons continue preſent in the very ſame form in which my imagination at firſt produced them. I find myſelf frequently in a ſtate betwixt ſleeping and waking, in which a number of pictures of every deſcription, often the ſtrangeſt forms, ſhew themſelves, change and vaniſh. In the year 1778, I was afflicted with a bilious fever, which, at times, though ſeldom, became ſo high as to produce delirium. Every day towards evening, the fever came on, and if I happened to ſhut my eyes at that time, I could perceive that the cold fit of the fever was beginning even before the ſenſation of cold was obſervable. This I knew by the diſtinct appearance of coloured pictures of leſs than half their natural ſize, which looked as if in frames. They were a ſet of landſcapes compoſed of trees, rocks, and other objects. If I kept my eyes ſhut, every minute ſome alteration took place in the repreſentation. Some figures vaniſhed, and others appeared. But if I opened my eyes all was gone; if I ſhut them again I had quite a different landſcape. This caſe was therefore entirely different from what afterwards in the year 1791, when the figure remained unchanged during the opening and ſhutting of the eyes. In the cold fit of the fever I ſometimes opened and ſhut my eyes every ſecond for the purpoſe of obſervation, and every time a different picture appeared replete with various ob-

jects

Narrative and remarks on spectres produced by nervous indisposition.

jects which had not the least resemblance with those that appeared before. These pictures presented themselves without interruption, as long as the cold fit of the fever lasted. They became fainter as soon as I began to grow warm, and when I was perfectly so, all were gone. When the cold fit of the fever was entirely past, no more pictures appeared; but if on the next day I could again see pictures when my eyes were shut, it was a certain sign that the cold fit was coming on. I must further observe, that when I either think deeply on a subject, or write attentively, particularly when I have exerted myself for some time, a thought frequently offers itself which has no connection with the work before me, and this at times in a manner so very lively, that it seems as if expressed in actual words.

This natural vivacity of imagination renders it less wonderful, that after a violent commotion of mind, a number of delusive pictures should appear for several weeks in succession. Their leaving me on the application of leeches, shews clearly that some anomaly in the circulation of the blood was connected with the appearance of those phantasms; though it may perhaps be too hasty a conclusion to seek for their cause in that alone. It seems likewise remarkable, that the beginning of the apparitions, after the disturbance in my mind was settled, as well as the alteration which took place when they finally left me, happened exactly at the time when digestion commenced. It is no less remarkable, that the apparitions before they entirely ceased, lost their intensity of colours; and that they did not vanish or change as formerly, but seemed gradually to dissolve into air.

Had I not been able to distinguish phantasms from phenomena, I must have been insane. Had I been fanatic or superstitious, I should have been terrified at my own phantasms, and probably might have been seized with some alarming disorder. Had I been attached to the marvellous, I should have sought to magnify my own importance, by asserting that I had seen spirits; and who could have disputed the facts with me? The year 1791 would perhaps have been the time to have given importance to these apparitions. In this case however, the advantage of sound philosophy, and deliberate observation may be seen. Both prevented me from becoming either a lunatic or an enthusiast; with nerves so strongly ex-

cited

cited, and blood ſo quick in circulation, either misfortune might have eaſily befallen me. But I conſidered the phantaſms that hovered around me as what they really were, namely, the effects of diſeaſe; and made them ſubſervient to my obſervations, becauſe I conſider obſervation and reflection as the baſis of all rational philoſophy.

Narrative and remarks on ſpectres produced by nervous indiſpoſition.

Our modern German philoſophers, will not allow that obſervation ought to be admitted in theoretical philoſophy. Hence aroſe Kants' Tranſcendental Idealiſm, which at laſt degenerated into the groſs enthuſiaſtic idealiſm; which is found in Fichte's writings. This philoſopher conſiders all external objects as our own productions. "What we con-"ſider as things independent of us are," according to him, "no more than our own creatures, which we fear, admire "and deſire; we believe our fate to be dependent on a "ſhadow, which the ſingle breath of a free being might "deſtroy." Theſe are Mr. Fichte's own words*.

The mere picture in the mind, without external experience, would never be ſufficient to afford us a convincing proof, whether we ſaw phenomena or phantaſms. The critical philoſophers maintain, that knowledge deduced from obſervation is merely empirick, and therefore not to be depended on; it is perhaps true that nature has aſſigned us no greater certainty than this reſpecting our ideas. But could we be truly conſcious of our grounds of reaſon, if the appearances called external, which follow laws that do not depend on the repreſentations in our mind, did not continually agree with thoſe repreſentations? Are we poſſeſſed of any other criterion? Does not the great theoretical philoſopher, when he ſees every thing yellow, conclude that his eye is jaundiced; or when every thing appears black to him, that his brain is affected? In theſe caſes he does not truſt his imagination or mental powers alone.

I may here apply the conſideration of the illuſions which I witneſſed. I am well aware that no general concluſions can be drawn from a ſingle inſtance; but ſtill the experience of a ſingle caſe, if accurately obſerved and faithfully deſcribed, is ſufficient to deſtroy hypotheſes which have too long been honoured with the name of ſyſtems.

* Fichte's Appeal, p. 44.

Narrative and remarks on ſpectres produced by nervous indiſpoſition.

According to Fichte, ſince during the ſituation I have above deſcribed, I was in other reſpects in the perfect uſe of my reaſon, as well as the perſons who were really about me; as the apparitions which I ſaw, as well as thoſe which are conſidered as realities, were the one as well as the other, my own productions:—Why then were my creatures of both kinds ſo eſſentially different?

My judgment ſhewed me this plainly, by concluſions founded on the previous courſe of obſervations. The greateſt modern idealiſts who depend ſo much on the confuſion in which they have involved themſelves by the ſuppoſed depth of their ſpeculations, will certainly never pretend that both perceptions were of the ſame nature; ſince if ſo, I could not have inveſtigated their difference? But by what means could this be done? I obſerved that real perſons followed in a determinate order, by external laws that do not depend on me, in an order that I myſelf muſt continually follow, as was evident from my ſenſe of conſciouſneſs. I could alſo lay hold of the real objects, as well as of myſelf. Neither of theſe circumſtances was, however, the caſe with the phantaſms; I had always found it ſo in the conſtant obſervation of myſelf, of the apparitions without me, and in my own conſciouſneſs.

The phantaſms, as well as the phenomena, no doubt, lay in my mind; but I am neceſſarily compelled to aſcribe to the latter, the ſame reality which I am obliged to aſcribe to myſelf: viz. ſomething that does not lie in my mind alone; ſomething that alſo exiſts without my mind; ſomething independent of my conſciouſneſs, which determines the nature of my idea; ſomething which we formerly uſed to call *the thing itſelf*, before the critical philoſophy ſo unjuſtly reprobated this unexceptionable term. On the contrary, however, I could not aſcribe this ſame reality to the illuſion; I could form no other concluſion, than that they originated in my internal conſciouſneſs alone; in a conſciouſneſs which was alſo diſordered, as I might juſtly conclude from the obſervations I made on myſelf. I repeat, that both the phenomena and the phantaſms exiſted in my mind: if I had not been able to diſtinguiſh between them, I muſt have been inſane. By what means could I diſtinguiſh, if I did not attribute reality to the former;—and that they poſſeſſed reality, I inferred from obſervations

ſervations to which I am ſtill inclined to give confidence, until Mr. Fichte can more clearly convince me that it ought in no caſe to be depended on.

III.

Analyſis of Ambergris; by Cit. Bouillon La Grange *.

It is an opinion now generally adopted, that ambergris is formed in the ſtomach of the cachalot, or ſpermaceti whale, *phyſeter macrocephalus*, and appears to be a product of its digeſtive faculties. Ambergris found in the ſtomach of the phyſeter macrocephalus.

Dr. Swediaur has ſhewn, in his inquiries into the nature and origin of ambergris, that the beaks of the cuttlefiſh, interſperſed throughout all the large pieces of ambergris, that are found ſwimming on the ſea, or caſt upon the ſhore, as well as thoſe extracted from the bellies of whales, belong to the ſpecies called by Linneus *ſepia octopodia.* The exiſtence of theſe beaks and other foreign ſubſtances in ambergris evidently proves it to have been originally in a ſoft or fluid ſtate. Dr. Swediaur aſſerts, that the whale, in the belly of which ambergris is found, is the ſame ſpecies as that from which ſpermaceti is extracted, which appears to be the *phyſeter macrocephalus* of Linneus; and feeds chiefly on the large ſpecies of cuttlefiſh. The ambergris is found in the inteſtinal canal of this fiſh; it is a ſource of diſeaſe to it †; and after it iſſues from the cavity in which it had been included, it gradually acquires the ſolidity it is known to poſſeſs. Beaks of cuttle-fiſh found in all the large pieces; whence it muſt once have been ſoft, if not fluid. The ſpermaceti whale that produces it. Occaſions a diſeaſe in the fiſh? becomes ſolid after its excluſion.

Ambergris is found in the Indian Seas, near the Moluccas, Maldivia Iſlands, and Madagaſcar, on the coaſts of China and Japan, and from Jolo to the Philippine Iſlands. It is frequently collected on the ſhores of the Iſland of Maragnan, or of Brazil; but more commonly on thoſe of Africa, toward cape Blanco, the gulf of Arguin, the bay of Portendie, and on ſome other Iſlands, that extend from Moſambique to the Red Sea. Found on the ſhores of the Indian Ocean and its iſlands. Of Brazil, and alſo of Africa.

* *Annales de Chimie*, No. 139. or XLVII. 68.

† Is it not rather the effect, than the cauſe of diſeaſe? J. C.

The inhabitants of the Samballas feek for it on the fhore after ftorms by the fmell.

From the accounts of various travellers, the inhabitants of the Samballas feek for it in a fingular manner: they hunt it by fcent. After a ftorm they run along the fhore, and if any ambergris be thrown up, they findit by the fmell. There are certain birds and other animals on thofe coafts, that are very fond of ambergris, and, attracted from a diftance by its fmell, they fearch for it to eat.

Certain birds and other animals fond of it.

Certainly a vegetable production.

There is no doubt, that ambergris is a vegetable production. Many fubftances refemble it greatly in fmell, fuch as the excrements of mammiferous animals, particularly thofe of the ox and the pig. I have found, that cowdung dried in the fun, has a fmell much like that of ambergris, and even of mufk, whence in fome countries this fubftance, fo prepared, has received the name of *native mufk.*

Excrements of fome animals, particularly of the ox and pig, refemble it in fmell.

Cowdung called in fome places native mufk.

External qualities of ambergris.

Ambergris, *ambra grifea,* is a light fubftance, fwimming on water, folid, opaque, of an afhen gray colour ftreaked with white and yellowifh brown, flightly odoriferous, its odour difplaying itfelf more as it grows old, or when it is mixed with mufk or other aromata, as is done in preparing perfumes or odoriferous waters.

Its odour more powerful as it grows old, or when mixed with other perfumes.

Marks of good ambergris.

In its natural ftate good ambergris is known by adhering like wax to the edge of a knife with which it is fcraped, retaining the impreffion of the teeth or nails, and emitting a fat odoriferous liquid on being penetrated with a hot needle. Though folid, and in general brittle, it is not hard enough to take a polifh; but on rubbing it with the nail it becomes as fmooth as hard foap.

The older chemifts claffed it among the bitumens.

Geoffroy, Neumann, Grim, and Brow, have claffed ambergris among the bitumens. The analyfis made of it by thefe chemifts was inadequate to determine its nature. Ambergris, fays Geoffroy, melts into a refin of a yellow or gold colour; kindles, and burns with flame. Spirit of wine does not diffolve it entirely; a black fubftance like pitch being left, on which it does not act. When it is diffolved, it lets fall after fome time a white cloudy fediment, which gradually coagulates, and grows thicker and thicker. This *coagulum,* on drying, changes to a fhining foliated earth, nowife different from fpermaceti.

Geoffroy's analyfis of it by alcohol,

and by diftillation.

On diftillation, according to the fame chemift, ambergris yields at firft an infipid phlegm, then an acid fpirit or liquor

and

and a very odoriferous yellow oil, with a ſmall portion of a volatile acidoſaline ſalt; and laſtly, a ſhining black bituminous ſubſtance remains at the bottom of the retort. Hence we ſee this analyſis, which does not differ from thoſe related by all other chemiſts, requires to be reviſed, in order to give us determinate ideas of the nature of this ſingular ſubſtance.

This analyſis inſufficient.

It is perhaps neceſſary to appriſe thoſe, who wiſh to repeat theſe experiments, that they ſhould pay great attention to the choice of the ambergris. Many varieties are found in the ſhops, the different kinds of which are diſtinguiſhed by their price. No doubt this ſubſtance is fabricated, as caſtor is in ſome parts of Germany. Bayen aſſured me, that he had ſeen it made at Frankfort; and it is well known that this father of chemiſtry ſaw clearly, and that his memory was not apt to deceive him; and, what is very rare among travellers, that he never told a lie.

Neceſſary to be very careful in chooſing it.

Many varieties of it in the ſhops

Fabricated by art, as Bayen ſaw at Frankfort

I have examined ſeveral ſpecimens of the ambergris of the ſhops: ſome varied in ſpecific gravity, were more or leſs deep in colour, had very little ſmell, and were flexible; others were of an aſhen gray colour, and tolerably hard; and ſome were almoſt ſtony, ſcarcely at all ſoluble in alcohol, and void of ſmell.

Differences of theſe varieties.

The ambergris I analyſed was not purchaſed from the ſhops; and, on comparing it with that in the cabinet of the Muſeum, I could find no difference, either in colour or in ſmell.

Phyſical Properties.

It is of an aſhen gray colour, internally variegated with a few yellow ſtreaks, of a ſweet and pleaſing ſmell, ſoftening between the fingers; when reduced to a fine powder it is of a deeper colour; pounded in a glaſs mortar it agglutinates, and adheres to the peſtle.

Its colour, ſmell, texture,

Of a flat and almoſt inſipid taſte, exhibiting the ſame appearances as wax when chewed between the teeth.

taſte,

Its ſpecific gravity is to that of water as 844 or 849 to 1000.

ſpecific gravity.

According to Briſſon, the ſpecific gravity of ambergris is 9263; the weight of the French cubic inch, 4 gros 58 grs.; that of the cubic foot, 64 lbs. 14 oz. 3 gr. 47 grs. *

* The ſpecimens of ambergris, on which Briſſon made his experiments, were taken from the king's collection.

The

The ſpecific gravity of the blackiſh gray ambergris 7803; the weight of the cubic inch, 4 gros 3 grs.; that of the cubic foot, 54 lbs. 9 oz. 7 gr. 35 grs.

Chemical Properties.

It burns entirely away.

Experiment I. Ambergris burns, and is entirely diſſipated, when placed on a red hot coal. It leaves behind an agreeable ſmell.

Melts with a leſs degree of heat,

If the combuſtion be conducted more ſlowly, in a crucible of platina, the ambergris melts, diffuſing the ſame ſmell. The ſmell of a fatty ſubſtance may be diſtinguiſhed likewiſe.

Nothing remains in the crucible, but a greaſy black ſpot.

and is then a ſhining brown fluid.

50° of Reaumur's thermometer are ſufficient to melt it, and a ſhining brown fluid is thus obtained.

Becomes volatile at 80° R.

At 80° it is volatilized in the form of a white vapour.

Its ſmell indicates an acid.

Exp. II. The ſmell perceived during its volatilization having led me to ſuſpect the preſence of an acid analogous to that of balſams, an experiment was made to aſcertain this.

This acid detected by evaporating it under a bell,

A bit of ambergris was placed in a china capſule, covered with a bell, in which was ſuſpended ſome litmus paper. This apparatus being placed on a ſand-heat, the temperature was raiſed ſufficiently to volatilize the ambergris, and the paper was very quickly reddened. Nothing now remained but to determine the nature of the acid; and for this purpoſe

and proved to be the benzoic.

Scheele's proceſs for extracting the acid of Benjamin was adopted.

The product was examined, and left no doubt of their analogy.

On diſtillation,

Exp. III. The analyſis by diſtillation in a retort added nothing to the knowledge we already poſſeſſed of the nature of ambergris.

it gives out a whitiſh acid liquor, with a light oil, and leaves a bulky coal.

A gentle heat melted it: on raiſing the fire it was decompoſed, and there paſſed over into the receiver a whitiſh acid liquor with a white oil, partly ſoluble in alcohol, which gave it a yellow colour. In the retort remained a light and very bulky coal.

Imparts neither taſte nor ſmell to cold water,

Exp. IV. Ambergris ſwims on water, and is not penetrated by it when cold. It imparts to it neither taſte nor ſmell,

Boiling

Boiling water is equally incapable of altering its properties. In this degree of heat the ambergris melts, and appears in the form of a brownifh oily fluid; and a fmall quantity of black matter, infoluble in alcohol, feparates from it. The filtered liquor has neither colour nor fmell, it has however a flightly bitterifh tafte.

and to boiling water it gives only a flight bitter tafte.

It is only in confequence of the temperature therefore that the ambergris melts, fince on this being lowered it refumes the fame properties as before.

Exp. V. Acids in general have little action upon ambergris. Thefe agents likewife do not enable us to difcover the conftituent parts of this compound fubftance.

Acids act but feebly on it.

Dilute fulphuric acid effects no change in it. The concentrated acid expofes a little oxide of carbon.

Sulphuric.

The fame phenomena are produced by the muriatic and oxigenated muriatic acid.

Muriatic.

The nitric acid, at 18°, diftilled over this fubftance in the pneumato-chemical apparatus, produces nitrous gas, carbonic acid, and azote gas.

Nitric produces nitrous gas, carbonic acid, and azote gas;

The azote gas arifes no doubt from the decompofition of fome animal matters, accidentally mixed with the ambergris, as may be obferved in the examination of fome pieces.

After the extraction of the elaftic fluids, a thick liquor, inclining to a yellow colour, was found in the retort: this, on bringing it to a foft confiftency, flightly fwelled up; and being evaporated to drynefs, in a porcelain capfule, what remained was a dry, bitter fubftance, of a golden yellow hue, fhining and tranfparent, and exhibiting properties analogous to thofe of refins.

and leaves a fubftance analogous to refins.

Exp. VI. Alcalis combine with ambergris, and form with it foluble foaps.

Alcalis form foap with ambergris.

Into a crucible of platina were put one gramme, 592 (30 fr. grs.) of ambergris, with 531 thoufandth of a gramme (10grs.) of pure potafh; it was gently heated; the mixture melted, without exhibiting any figns of the prefence of ammonia; on cooling a homogeneal brownifh mafs was obtained.

On this were poured 30 grammes (one fr. ounce) of diftilled water, which diffolved part of it. The folution was very alcaline.

The undiffolved portion remained in a foft tenacious mafs, which adhered to the fingers when warm.

A larger quantity of water was added, and the whole was diffolved.

Cauftic potafh does not facilitate its folution in cold water.

Cauftic potafh triturated for fome time in a mortar with ambergris does not facilitate its folution in water.

Ammonia diffolves it with the aid of heat.

Ammoniac does not act on ambergris cold, but when heated diffolves it; the mixture gradually becomes brown, and on evaporation yields a glutinous faponaceous fubftance, in all refpects fimilar to that obtained by means of potafh.

It is foluble in the fixed oils,

Exp. VII. The fixed oils, as thofe of rape, olive, &c. diffolve amber with the affiftance of heat in a very fhort time; the folution is yellow and tranfparent, and becomes brown on being evaporated.

and in the volatile oils.

Exp. VIII. Volatile oils likewife diffolve ambergris.

Thofe of turpentine, favine, and hyffop, exhibit the fame appearances. The folution affifted by heat takes place pretty readily.

On evaporating the folution a red magma is left, which is foluble in alcohol.

On evaporation a thick red magma is produced, incapable of complete deficcation, burning on the coals, and emitting a denfe fmoak, of a fmell refembling that of the ambergris. Alcohol diffolved this fubftance, and thence acquired a golden yellow colour, but it was precipitated from it by means of water.

Old volatile oils will not diffolve it.

If volatile oils be too old, they will not completely diffolve it, even with the help of long continued heat.

Soluble in ether.

Exp. IX. It diffolves very quickly in ether, even cold.

Alcohol feparates its conftituent parts.

Exp. X. The folution of ambergris by alcohol is the only one that is really capable of affording us any certain refults. Its conftituent parts may be feparated by it in fuch a manner, that on reuniting them a compound is obtained, the qualities of which came very near thofe of the original fubftance.

Part diffolved in alcohol without heat;

3.821 grammes (one drachm) of ambergris were reduced to powder, put into a phial, and 61.143 grammes, (two ounces) of rectified alcohol were poured on them. A maceration of twenty four hours was fufficient to give the alcohol a deep yellow colour; it was filtered, and a frefh quantity of alcohol was poured on the undiffolved portion. The folution was facilitated by increafing the temperature. The whole of the ambergris being diffolved, except a fmall quantity of black matter, the liquor was filtered while hot. It paffed through the filter clear; but on cooling there feparated from it a light pale yellow fubftance, part of which adhered to the fides of the veffel.

another part by means of heat; leaving a little black matter; and feparating when cold.

The

The firft folution in alcohol made without heat, and that which was poured off from the precipitate, were mixed together, and evaporated to the confiftence of an extract: it was then of a reddifh yellow colour, adhered to the fingers, had an agreeable fmell, and a pleafant tafte. The evaporation being continued to drynefs, it appeared fhining and tranfparent, grew foft between the fingers, and burnt in the fame manner as refins.

This folution mixed and evaporated.

left a refinous fubftance.

The experiment was repeated, to determine the characters of thefe two fubftances more pofitively.

For this purpofe ambergris was left to macerate in alcohol twenty-four hours as before; it was then filtered, and a frefh quantity of alcohol was added to the refiduum, which was macerated in the fame manner. The fecond liquor was lefs coloured than the firft. A third portion of alcohol being poured on what was left undiffolved; its colour was fcarcely altered. The flight action of the alcohol on this refiduum feemed to indicate, that it was no farther foluble in this menftrum; but I quickly found the contrary. I heated the mixture, and the whole was inftantly diffolved, leaving about 212 thoufandths of a gramme, (four grains) only of a black powder, which was nothing but oxide of carbone. The folution was filtered hot, and on cooling a whitifh yellow glutinous fubftance was depofited, which was feparated from the tincture.

The experiment repeated.

The black powder oxide of carbone.

This experiment fhows us the poffibility of feparating by means of alcohol three very diftinct fubftances; the firft foluble in it cold; the fecond, by means of heat; and the third infoluble, which remains in the form of powder.

Thus three different fubftances feparated.

To determine the characters of the firft two fubftances, the tincture made without heat was firft evaporated to drynefs; when there remained in the capfule 1.167 grammes (22 grains) of a brown fubftance, dry and fhining in its fracture, unalterable in the air, and growing foft with a gentle heat; 15° were fufficient to give it a tenacious and glutinous confiftence; and being put on red-hot coals it was completely volatilized. If this experiment be made in a filver fpoon, the volatilization takes place with the fame rapidity, an odoriferous fmell is diffufed around, and no coally refiduum is left.

The firft examined.

Sufpecting this fubftance might be in fome refpect analogous to the refin obtained from propolis by Cit. Vauquelin, I inftituted a comparifon between them, and found the following differences;

Differs from the refin obtained from propolis

1ft.

In three respects.

1ft. It melts much more flowly; 2dly, it diffufes a denfe odoriferous vapour, refembling a little the fmell of honey; 3dly, it fwells up, and leaves a very bulky coal.

Is a true refin.

Finally, this firft fubftance obtained from ambergris, which may be confidered as a true refin, is foluble in alcohol, and is precipitated by water. The folution reddens litmus paper, which proves too, that the alcohol diffolves the benzoic acid previoufly detected, either by burning the ambergris under a bell, or by treating it with lime.

Examination of the fecond fubftance.

Nothing now remains, but to examine the product obtained by heated alcohol, after the refin is extracted by maceration.

I have faid above, that there feparated from the alcohol by refrigeration a fubftance, part of which fubfided to the bottom of the veffel, and part adhered to the fides.

Being feparated from the liquor, and properly dried, it remains a little bulky and light. Under the preffure of the finger it contracts and crumbles, but it is foon lengthened out and foftened by the heat. It has a laminated texture, if it be fuffered to cool flowly.

It retains between its particles a little water and alcohol, which may be feparated by keeping it a fhort time in fufion. When melted over again it is much whiter than before, and no longer exhibits its former granulated texture. In fine, I have difcerned in it all the properties of the adipocerous fubftance, difcovered by Cit. Fourcroy in the fatty matter of dead bodies, and the properties of which he has defcribed in a paper publifhed in the 8th volume of the Annals of Chemiftry.

Its properties the fame with thofe of the adipocerous fubftance found in the fatty matter of dead bodies.

From 3.821 grammes, (72 grains) of ambergris, 2.016 grammes, (38 grains) of adipocerous matter may be obtained.

Recapitulation.

From thefe experiments it appears we may conclude:

Recapitulation.

1ft. That ambergris is a compound fubftance, which burns, and may be entirely volatilized.

2dly, that on diftilling it alone we obtain from it a flightly acid liquor, and an oil partly foluble in alcohol, and of an empyreumatic fmell.

3dly. That by fublimation, or by the procefs of Scheele, benzoic acid may be extracted from it.

4thly. That water does not act upon it.

5thly,

5thly. That by means of nitric acid a matter analogous to resins, mixed with the adipocerous substance, is extracted from it.

6thly. That the concentrated sulphuric, muriatic, and oxigenated muriatic acid, convert it to a coal, without dissolving it.

7thly. That with alcalis it forms a saponaceous compound.

8thly. That fixed oils, volatile oils, ether and alcohol, are the true solvents of ambergris.

9thly. And lastly, that alcohol affords the means of separating its constituent parts in the following proportions.

Its constituent parts.

Adipocerous matter - - - -	2.016 grammes.
Resin - - - - - - - -	1.167
Benzoic acid - - - - - -	0.425
Coally matter - - - - -	0.212
	3.820.

IV.

An Account of some Stones said to have fallen on the Earth in France; and of a Lump of native Iron, said to have fallen in India. By the Right Hon. CHARLES GREVILLE, *F. R. S**.

That stony and metallic bodies have fallen on the earth is fully established.

THE experiments and observations made by Edward Howard, Esq. on certain stony and metalline substances said to have fallen on the earth, and the accurate description which the Count de Bournon has given of those substances, have, in my opinion, fully established the following fact, namely, that a number of stones asserted to have fallen under similar circumstances, have precisely the same characters.

The stones from Benares, that from Yorkshire, that from Sienna, and that from Bohemia, were the whole which had then been seen in England. They all contained pyrites of a peculiar character: they all had a coating of black oxide of iron: they all contained an alloy of iron and nickel; and the earths which served to them as a sort of connecting medium, corresponded in their nature, and nearly in their proportions.

Three new specimens from France.

Since the publication of Mr. Howard's and Count de Bournon's observations, I have received from France three

* From the Philos. Trans. 1803.

additional

Remarkable history.

additional ſpecimens. Monſieur St. Amand very obligingly divided with me a ſpecimen he had broken from a ſtone of about 15 inches diameter, preſerved in the Muſeum of Bourdeaux, which ſtone fell near Roqueford, in the Landes, on the 20th Auguſt, 1789, during the exploſion of a meteor; it broke through the roof of a cottage, and killed a herdſman and ſome cattle. M. St. Amand alſo gave me part of a ſtone he had preſerved in his collection ever ſince the year 1790, when a ſhower of ſtones, weighing from ½ an ounce to 15 and 25 pounds each, fell in the pariſhes of Grange and Creon, and alſo in the pariſh of Juliac, in Armagnac; which fact was, at the time, verified by Duby, Mayor of Armile, and publiſhed by Bertholon, in the *Journal des Sciences utiles de Montpellier*, in the year 1790.

The third ſpecimen, I owe tothe Marquis de Dree; it is a fragment, broken from a ſtone of 22 pounds weight, which fell near the village of Salles, not far from Villefranche in Burgundy, on the 12th of March, 1798; this was alſo accompanied by a meteor.

These three ſpecimens agree in characterwith the others.

I content myſelf with the mere recital of the facts, in confirmation of the obſervations preſented to the Society, as theſe three additional ſpecimens have preciſely the ſame characters, texture, and appearance, as the others in my collection; and are ſcarcely, by the eye, to be diſtinguiſhed from them.

I ſhould not, perhaps, have troubled the Society with this account, as my friend the Marquis de Dree, whoſe knowledge in mineralogy peculiarly qualifies him to inveſtigate theſe ſubjects, has given me hopes of ſeeing his obſervations on them publiſhed; but a new evidence has lately fallen into my hands, and is the only one I have met with that aſcertains the origin of native iron, which from analyſis, had been ſuſpected to have a common origin with the ſtones fallen on the earth. Conversing with Colonel Kirkpatrick, whoſe reſearches have embraced both the literature and politics of India, and whoſe talents had placed him in very important ſituations in various parts of India, I inquired whether he had ever heard of any inſtances ſimilar to the exploſion of the meteor at Benares in 1798. He told me, he could not recollect having heard or read of any other inſtance, excepting one in the Memoirs written by the Emperor Jehangire, and of that he did not recollect the particulars. A few days after, having found the

Metallic ſtone that fell in India nearly two centuries ago.

paſſage

paſſage in the original Perſian, he was ſo obliging as to tranſlate it. I conſider it as an authentic fact; for the Emperor Jehangire was not a prince on whom his courtiers would idly venture to impoſe; and there can be little probability that an Aumil of a diſtrict ſhould invent ſuch a ſtory, or be able to produce a ſubſtance apparently like iron, but which, on trial, differed from manufactured iron. Colonel Kirkpatrick's tranſlation I have obtained his leave to communicate, with his atteſtation, to the Royal Society.

Extract from the Memoirs of the Emperor Jehangire, written (in Perſian) by himſelf, and tranſlated by Colonel Kirkpatrick.

Narrative written by the Emperor Jehangire of a metallic ſtone that fell in the year 1620.

A. H. 1030, *or 16th year of the reign.*—The following is among the extraordinary occurrences of this period.

Early on the 30th of Furverdeen, of the preſent year *, and in the Eaſtern quarter, [of the heavens] there aroſe in one of the villages of the Purgunnah of Jalindher †, ſuch a great and tremendous noiſe as had nearly, by its dreadful nature, deprived the inhabitants of the place of their ſenſes. During this noiſe, a luminous body [was obſerved] to fall from above on the earth, ſuggeſting to the beholders the idea that the firmament was raining fire. In a ſhort time, the noiſe having ſubſided, and the inhabitants having recovered from their alarm, a courier was diſpatched [by them] to Mahommed Syeed, the Aumil ‡ of the aforeſaid Purgunnah, to advertiſe him of this event. The Aumil, inſtantly mounting, [his horſe,] proceeded to the ſpot, [where the luminous body had fallen]. Here he perceived the earth, to the extent of ten or twelve guz §, in length and breadth, to be burnt to ſuch a degree, that not the leaſt trace of verdure, or a blade of graſs remained; nor had the heat [which had been communicated to it] yet ſubſided entirely.

* The firſt of Furverdeen of this year, (A. H. 1030,) correſponded with Saturday, the 27th of Rubbi ul Akhir; conſequently, the 30th of Furverdeen fell on the 26th of Jummad ul Ouwul, or A. D. 1620.

† A purgunnah is a territorial diviſion, of arbitrary extent. The purgunnah of *Jalindher* is ſituated in the Punjaub, and about 100 miles S E. of Lahore.

‡ Aumil is a manager or fiſcal ſuperintendant of a diſtrict.

§ A gus is rather leſs than a yard.

Mahommed

Mahommed Syeed hereupon directed the aforesaid ſpace of ground to be dug up; when, the deeper it was dug the greater was the heat of it found to be. At length, a lump of iron made its appearance, the heat of which was ſo violent, that one might have ſuppoſed it to have been taken from a furnace. After ſome time it became cold; when the Aumil conveyed it to his own habitation, from whence he afterwards diſpatched it, in a ſealed bag, to court.

It weighed upwards of four pounds, and was brittle.

Here I had [this ſubſtance] weighed in my preſence. Its weight was one hundred and ſixty tolahs.* I committed it to a ſkilful artiſan, with orders to make of it a ſabre, a knife, and a dagger. The workman [ſoon] reported, that the ſubſtance was *not malleable, but ſhivered into pieces under the hammer.*†

Upon this, I ordered it to be mixed with other iron. Conformably to my orders, three parts of the *iron of lightning*‡ were mixed with one part of common iron; and from the mixture were made two ſabres, one knife, and one dagger.

With the addition of one part of common iron to three parts of the metallic ſtone, excellent blades were made.

By the addition of the common iron, the [new] ſubſtance acquired a [fine] temper; the blade [fabricated from it] proving as elaſtic as the moſt genuine blades of Ulmanny,§ and of the South, and bending, like them, without leaving any mark of the bend. I had them tried in my preſence, and found them cut excellently; as well [indeed] as the beſt genuine ſabres. One of theſe ſabres I named *Katai*, or *the cutter*; and the other *Burk-ſeriſht*, or the *lightning-natured*.

A poet|| compoſed and preſented to me, on this occaſion, the following tetraſtich.

"This earth has attained order and regularity through the
"Emperor Jehangire:
"In his time fell *raw* iron from lightning:
"That iron was, by his world-ſubduing authority.
"Converted into a dagger, a knife, and two ſabres."

* A tolah is about 180 grains, Troy weight.

† Literally, "it did not ſtand beneath the hammer, but fell to pieces.

‡ This expreſſion is equivalent to our term *thunder-bolt*.

§ The name of the place here deſigned is doubtful.

|| The poet is named in the original; but the name is not perfectly legible.

The

The chronogram of this occurrence is contained in the words* (   ) which fignify " the flame of the imperial lightning;" and give the year (of the Hegera) 1030.

N. B. The foregoing tranflation (which is nearly literal) has been made from a manufcript that has been feveral years in my poffeffion; and which, although without date, bears marks of having been written at a remote period.

WM. KIRKPATRICK.

V.

Analyfis of the Natrolite. *By* KLAPROTH.†

Natrolite found at Hogau, in Suabia.

THE foffil which forms the fubject of this analyfis, and to which I give the name of *natrolite*, for reafons to be ftated hereafter, is found at Högau in Suabia, on the borders of Switzerland. It is depofited in the crevices, or clefts and cavities of the fonorous porphyry. (Klinginftein Porphyr) from having a found, nearly metallic, which form the mountains and rocks of Hohentwiel, Hohenkrähen, and Mägdeberg.

Its colour a dirty yellow.

Its properties.

The colour of this foffil is a dirty ochreaceous yellow, approaching fometimes to an Ifabella yellow, at other times to a yellowifh brown, interfected with concentric white lines. It is compact its internal fracture has a filky luftre. It breaks into wedge-like pieces, the edges of which poffefs little tranparency; it is not very hard, extremely brittle, and of=2,200 fpecific gravity.

A.

Ignited in a filver crucible,

a. 100 grains of natrolite, after having been ignited for fome time in a filver crucible, loft nine grains. The figure of the ftone was retained, but its compactnefs was confiderably diminifhed.

Fufes quickly before the blow-pipe.

b. Before the blow-pipe on charcoal, natrolite fufes quickly into a tranfparent glafs, full of fmall air bubbles.

In a porcelain furnace melts into a brown glafs,

c. Natrolite placed in a clay crucible, and expofed to the heat of a porcelain furnace, fufed into a tranfparent glafs of a light brown colour.

* The Perfian characters are given in the Tranfactions. N.

† Abftract of an Effay in the memoirs of the Royal Academy of Sciences at Berlin, 1803, page 243.

d. In

with minute globules of iron in the surface.

d. In a charcoal crucible the mineral afforded the same product. The glass pearl exhibited on its surface minute globules of iron.

B.

Dissolved in muriatic acid.

a. 100 grains of finely levigated natrolite, were mixed into a pasty fluid with water, put into a flask, and digested in moderately strong muriatic acid. The solution was soon effected, and it exhibited a reddish yellow gelatinous mass. After diluting it copiously with water, and continuing the digestion, the siliceous earth separated, which, being collected and dried, weighed 48 grains.

The siliceous earth being precipitated by water,

cubic crystals were obtained.

b. The fluid obtained in the last process on evaporation, yielded cubic crystals. The remaining fluid being further evaporated to dryness, the dry mass was pulverifed, and digested with a gentle heat in alcohol. Having suffered the alcoholic solution to cool, a white saline powder was deposited; the alcohol was therefore decanted, and the powder collected, washed in spirit, and dried. The remaining alcoholic solution was afresh evaporated, a small quantity of the same saline powder became separated, which was added to that obtained before.

The dry mass digested in alcohol.

The residuum dissolved in water, and precipitated by ammonia.

c. The residue of the process *b* insoluble in alcohol, was dissolved in water. On adding to this solution liquid ammonia, a light flocculent precipitate became deposited. This being separated by the filtre, the fluid which passed through, was evaporated by a gentle heat. The salt obtained, weighed when perfectly dry, 31¼ grains.

The solution in alcohol decomposed by ammonia.

d. The alcoholic solution *b* (which from other experiments, was known already to contain nothing but alumine and oxide of iron) after being diluted with water, was decomposed by liquid ammonia, and the precipitate collected and dried. The fluid, from which this precipitate had been separated, was evaporated, and the mass strongly heated, so as to volatilize the muriate of ammonia that had been formed, when there remained two grains of salt, which being dissolved in water, yielded cubical crystals.

The precipitates digested in solution of potash.

e. The precipitate obtained by means of liquid ammonia *d*, together with that before produced *c*, were put into a solution of pot-ash, and digested with that fluid. A solution was effected, and oxide of iron separated, which, after being ignited, weighed 1¼ grains.

f. The

f. The alcaline solution *e* was mingled with muriatic acid, till the precipitate produced, became redissolved, and was then decomposed by carbonate of soda. The precipitate obtained after being washed, dried, and ignited, weighed 24¼ grains. It was alumine.

Alumine separated from the alcaline solution.

g. It remained still to examine the alcaline part of the fossil, which produced with muriatic acid the 31¼ gr. *c.* and the two grains *d.* Taste, figure of cryftals, and chemical reagents, proved it to be muriate of soda. A solution of it in water, mingled with a concentrated solution of tartareous acid, did not produce tartarite of potash. Another part of the solution, after being decomposed by sulphuric acid, yielded sulphate of soda.

The alcaline part of the fossil, soda.

Having ascertained by experiments that 100 parts of absolutely pure carbonate of soda *, dried in a heat of ignition, when saturated with muriatic acid, lost 41 parts by weight of carbonic acid, and yielded 120½ parts of dry muriate of soda (the deficcation of which was not continued to decrepitation) we may conclude, that the above 33¼ grains of muriate of soda contained 16½ of soda.

100 parts of the natrolite consequently yielded :

Component parts of natrolite.

Siliceous earth	B.	*a*	48. grs.
Alumine		*f*	24.25
Oxide of iron		*e*	1.75
Soda		*g*	16.50
Water	A.	*a*	9.—
			99.50

The small number of fossils which contain soda, is therefore augmented by one more. That soda was contained in this stone might perhaps have been expected, on account of its forming frequently the matrix of the sonorous porphyry, which,

* In order to obtain perfectly pure carbonate of soda, I dissolve common carbonate of soda in water, and saturate this solution with nitric acid, taking care that the acid is a little in excess. I then separate the sulphuric acid by nitrate of barytes, and the muriatic acid by nitrate of silver. The fluid thus purified I evaporate to dryness, and fuse the nitrate of soda obtained, and decompose it by detonation with charcoal. I then dissolve the residue, filter, and crystallise the carbonate of soda.

Pure carbonate of soda.

 it

it is now known, always contains this alcali, but as the quantity of soda contained in this fossil, is twice as large as that which exists in the sonorous porphyry, I have not hesitated at giving it the name of *natrolite*.

VI.

*On the Employment of Aerostatic Machines in the Military Science, and for the Construction of Geographical Plans.** *By* Citizen A. F. Lomet.†

Present state of aerostation.

THE aerostatic art is still in its infancy; and whatever progress may have been already made in it, it is impossible to foresee all the resources it may afford, or to determine the limits of its utility. Time and experience must fix our opinions respecting it; but it is of consequence to obtain the assistance of learned men and artists in this interesting pursuit; and as the smallest investigations of this nature are generally too expensive for individuals, it is necessary that the government should support an establishment particularly devoted to the practice and improvement of the processes which constitute it.

Its improvement should be promoted by government.

Advantages to be derived from its use in war.

Aerostats will furnish, in presence of an enemy, one or more points of observation at pleasure, from which the positions he occupies may be reconnoitred, his movements studied, and his manœuvres judged of in the gross, or appreciated in the most minute detail. It may be presumed that these machines will become of the most indispensable utility in war, because they supply it with an extraordinary means, hitherto unknown, of making observations, which may in an instant determine the fate of battles, secure the dispositions for a vigorous defence, or at least point out the moment and the most convenient outlets for a retreat; and more particularly to draw attention to the advantages which an army may derive from balloons, it will be sufficient to remember the happy use made of them at the battle of Fleurus.

Instance.

* Adjutant-commandant, formerly keeper of the collection of models belonging to the Polytechnic School, and now at the head of the sixth division of the war department. (Military operations and movement of the troops.)

† From Journal de l'Ecole Polytechnique, Tome IV. p. 252.

The

The Committee of Public Safety, and afterwards the Executive Directory, thought that the application of aeroftats to military inquiries of every defcription ought to be ftudied and practifed during peace. They were alfo defirous, that they might be employed in the conftruction of geographical plans, or at leaft in afcertaining the intermediate particulars of the territory between the points which had been geometrically determined. Having been charged with the experiments relative to thefe different applications, I purpofe giving an account of the principal refults.

Experiments undertaken by order of the French government.

The intention, from the firft afcents, was to meafure the angle formed by the vifual rays falling on the eye of the aerial obferver, from feveral determinate points on the earth. The unavoidable motion of the aeroftat preventing the ufe of the graphometer in this operation, a recipiangle was at firft fubftituted, fufpended like a mariner's compafs, by the affiftance of which, it was hoped the meafure of the angles would be eafily taken, and particularly that they would be obtained with immediate relation to a horizontal plane. This attempt not having fucceeded, it was neceffary in future to make ufe of a fextant.

Firft attempts at meafuring the angle of the vifual rays.

This inftrument was every thing that could be defired for celerity, as well as for the facility and precifion of the obfervations, but it has this inconvenience, in the cafe in hand, that it only fhews the angle on a plane inclined to the horizon; and moreover, in its ordinary conftruction it furnifhes no means of noticing this inclination. The perpetual agitation of the machine is another fource of error; in fact, an aeroftat, kept elevated and held by cords, is continually changing pofition; it moves in fpace, defcribing alternate ellipfes, the curvature of which is modified to infinity, according to the violence of the wind, the elafticity of the cords, and the fituation of the places to which it is faftened. It leaves then no trace of its variations, and does not permit the obferver which it fupports, to add to the meafure of any angle whatfoever, that of the two angles neceffary to connect the firft with the plane of the horizon.

The ufe of the fextant is not free from error.

Neverthelefs, for plans relative to the generality of military inquiries, and in all cafes where a fketch of the figure of the earth is fufficient without attending to flight inaccuracies in diftances, fimple obfervations, made with the fextant, will anfwer the purpofe, and furnifh the means of operating with

But is neverthelefs applicable in many cafes.

facility over a vaſt extent of territory, ſecure from the attempts of an enemy. But it is not equally ſerviceable in operations which require a rigorous exactneſs, and in which it is requiſite to connect the angles with the centre of the ſtation, and with the plan of the horizon.

Additional apparatus to render the ſextant capable of indicating *all* the required angles.

The following is the mode in which I have endeavoured to fulfil the various objects:

The angles neceſſary for connecting the poſition of two objects with the centre of the ſtation and the plane of the horizon, are, 1ſt. the angle comprehended between the rays falling on the eye of the obſerver from theſe points; 2d. the angles formed by each of theſe rays with the perpendicular.— We have ſeen, that it is impoſſible for the aeroſtatic obſerver to mark theſe three angles by taking them after each other; but if their meaſure were inſtantaneous, the difficulty would be overcome. This would therefore be the caſe if an inſtrument could be deviſed, which would give theſe three angles at once by a ſingle obſervation; and as the ſextant already ſhows the angle comprehended between the rays, the object in view is to add the neceſſary parts to that inſtrument for obtaining the other two at the ſame time.

Preliminary obſervations.

Let B A C (*Plate* X.) be the angle formed by the viſual rays A B, A C; falling on the mirror A of the ſextant from the objects B and C; if the index A D be moved until the image of the object C reflected by the mirror A placed on the index coincides by double reflection on the mirror L, with the point where the object B is ſeen; and if they be both perceived at the ſame time by the obſerver looking through the teleſcope P, it is known, *(by the Theory and Uſe of the Sextant)* 1ſt, That the angle D A E, compriſed between the index A D and the fixed radius or line of zero A E of the inſtrument, is always equal half the angle B A C, the meaſure of which is required: 2d, That the line R B, which is ſuppoſed to paſs through the axis of the teleſcope and the centre of the mirror L, is always directed to the point B, and is uſually taken for the ſide A B; the error ariſing from the ſmall diſtance A R being conſidered as nothing in practice: hence, if we ſuppoſe a viſual ray paſſing from the point R to the object C, the angles B R C and B A C may be reputed equal, and be taken indiſcriminately for each other.

This

This being premifed, if a ruler be placed in the direction R B, it may be confidered as in that of the fide A B, and if we can fucceed in fixing a fecond ruler in fuch a manner that the moveable index fhall carry it into the direction R C, at the inftant that the images of the two objects B and C are brought into one at the point L, it is evident that thefe two rulers will form between them the angle B R C, and confequently the angle B A C.

Method of afcertaining the angle formed by vifual rays.

To accomplifh this, let us fuppofe a fort of falfe fquare, S R Q, fituate in the plane of the inftrument, and moveable at its axis on a pivot fixed at the point R, at the interfection of the lines A R and R L; making the angle S R Q, comprifed between its arms, equal to the angle E R L, and the fide R S equal to the diftance A R. If now we fuppofe that the extremity S of the fide R S, is retained by a button in a groove M N, worked in the moveable index, the movement of it will be communicated to the falfe fquare in fuch a manner that the angle L R Q will always be equal to the angle B A C, and confequently the fide R Q will be placed in the requifite direction.

In fact, the triangle A R S being ifofceles in its form, the exterior angle $SRE = RAS + ASR. = 2RAS = BAC$; but S R Q being equal to E R L, if the common angle S R L be taken away, there will remain the angle $SRE = LRQ = BAC$.

Now let us fix under each of the two rulers R L and Q R a fmall graduated quadrant, fufpended in fuch a manner that it will place itfelf in the vertical plane of the fide correfponding to the angle obferved; let us affix to each of thefe quadrants a plummet, compofed of a ftiff arm moveable upon a pivot, and furnifhed with a nonius index and a weight, which gives it a conftant tendency to affume a vertical pofition, in whatfoever fituation the fextant may be placed; finally, let the whole be fo difpofed that the index of each plummet may be retained at will, at the divifion indicated on the limb by the effect of the fufpenfion, and this by means of a trigger, which can be pulled at the exact inftant of obferving the principal angle in the points of reflexion. It is evident that the fimultaneous action of the two rulers and the plummets will fhow the three angles fought, and that nothing remains but to reduce, by calculation, the angle B A C to the plane of the horizon.

Obfervation of the angles formed by the vifual rays and the perpendicular.

From which thofe on the plane on the horizon are obtained by calculation.

Thus

Success of the instrument.

This instrument, arranged in the manner here described, produced every desired effect in our experiments. The invention, as simple as it is happy, may become very important from the useful applications of which it is susceptible; and there is already reason to hope that, by bringing this to perfection, or by the formation of some analogous instrument hereafter, there will be a possibility of executing trigonometric operations with much correctness, by the assistance of aerostatic machines, notwithstanding their continual motion.

Probable future advantages.

Method of connecting all the observed angles in one common centre.

It was not enough to have discovered the means of connecting the angles with the plane of the horizon: it was still desirable, that all the angles observed during ascents in any one place, should have a relation to the common centre of observation. To accomplish this, it was necessary to keep a register, by some means, of the situation of the machine at the precise moment of measuring each of these angles. This was done by dropping from the aerostat, at that instant, a small stake, leaded and furnished with an iron point. This stake fell rapidly to the earth, into which it stuck, and marked a point corresponding to the summit of the angle measured. It was then easy to compare the position of this point, with that of one taken for the common centre of the observation, and thence to deduce the necessary corrections. It must however be noticed, that the stake, when abandoned to itself, acquires, at the instant of its fall, a compound motion which partakes of that of the aerostat, and consequently is not exactly vertical; but the error which results from this deviation is but slightly perceptible in practice.

Application of the process.

The calculations and ordinary processes of descriptive geometry will furnish all the means of making use of these different observations, and of expressing the results on paper; not only for their application to the construction of maps, but also to ascertain heights compared with the level: but we shall not in this place enter into any details on that head.

Inaccuracy to be expected from an inexperienced observer.

The observer engaged making these first experiments, soon perceives that the involuntary embarrassment, occasioned by the novelty of his situation, when he finds himself insulated and suspended at a height of seven or eight hundred metres, has a considerable influence both on the fidelity of his observations and on the time necessary for making them. All certainty depends, in fact, upon the confidence and readiness

of

of the obferver; and it cannot be concealed, that it may produce great inconveniences, becaufe this difficulty of operating opens wide limits for the errors which it is poffible to commit.

From this laft obfervation it will be feen: 1ft. That it is indifpenfably neceffary to have acquired a great aptitude for thefe fort of obfervations, to be able to execute them with precifion: 2d. That the proceffes juft defcribed are more fatisfactory in theory than they would be in thofe applications which require ftrict accuracy; and, that though there are fituations in which nothing can be fubftituted for their ufe in the conftruction of fome figured plans, it is at leaft proper never to ufe them in preference to thofe means of obfervation which are better known, and which can be employed with more certainty.

Conditions neceffary to make correct obfervations.

But it cannot be too often repeated, at the fame time, that aeroftats furnifh the means of giving the moft lively intereft to the delineation of the figure of the earth, in maps of all defcriptions; and that their ufe is of the greateft affiftance in the formation of plans, the outline of which may be fufficiently defined by a fimple eye-fketch. The aerial obferver, by difcovering a vaft extent of country, accuftoms himfelf fully to confider the general organifation of the afperities of the country, and even the particulars of its varieties, as well as the tone of colour, which appear to give a different character to each portion of territory. If this new method of obfervation be cultivated with affiduity, it will doubtlefs lead to a fenfible improvement in the art of drawing plans. But to benefit by the advantages which it affords, it is neceffary, that thofe who are deftined to this employment fhould join a profound knowledge of geometry to a great facility in defigning landfcapes. May they be convinced of the importance of this truth, and affure themfelves that no part of the plan can arrive at perfection, either ornamental or relative to civil and military purpofes, unlefs ftrict accuracy in the outline is accompanied by that fidelity of expreffion which is capable of producing in thofe who infpect the plans, all the ideas which the obferver had formed from the afpect of the country.

Advantages to be derived by employing balloons in furveying countries.

From all that has been faid, we may conclude, that the aeroftatic art combines properties no lefs valuable than unqueftionable in topographic operations and military refearches; that its perfection

Valuable properties poffeffed by thefe machines.

fection may produce new and invaluable properties; and that it would be equally impolitic to neglect the use of these machines, or not to obtain for them the information to be derived from reflection and experience. We shall terminate this memoir by an observation relative to their military uses. Our enemies would not fail to oppose to the creative industry of France, an industry of imitation: they would also have their balloons and ballooneers *(aerostiers.)* The influence of this innovation in war is of a nature to spread with rapidity, and it must soon cease to favour any nation exclusively. But even in this case the art of aerostatic machines will have acquired a higher degree of interest, because another element shall then be in the power of man, in which the efforts of genius and industry may be substituted instead of the inconsiderate devastations of force; and this observation ought to interest the friends of hu-manity in bringing them to perfection.

Military use of them.

VII.

Chemical Analysis and Properties of Arseniated Hidrogen Gas By Professor Tromsdorff.*

Scheele discovered an inflammable arseniated gas.

THE immortal Scheele, in his essay on arsenic and arsenic acid,† mentions an inflammable arseniated gazeous fluid, of which he says: "*Hinc intelligas, hunc aerem inflammabilem esse, regulumque arsenici solutum tenere.*" Scheele states, that he obtained this gas during the solution of tin in arsenic acid. The properties of this gas, as pointed out by him, are the following. Arseniated hidrogen gas is insoluble in water; it does not render lime-water turbid; mingled with atmospheric air, no diminution of bulk ensues; on bringing the flame of a taper in contact with this mixture, a loud detonation follows, and metallic arsenic is deposited. Interesting as the observations here pointed out must appear to every chemist, the object has been neglected by succeeding operators.

Its properties.

* From a memoir, read in the Royal Academy of Sciences at Berlin, 1803, p. 370.

† C. H. Scheele Om Arsenick och dess Syra; Kongl. Svensk. Vetenskaps Academiens *Handlingar*. Ar. 1775. V. xxxvi. 265.

Proust

Prouſt is the only philoſopher who mentions this gas:* he obtained it by digeſting arſenious acid and zinc, in dilute ſulphuric acid; on burning the gas, he obtained ſometimes arſenious, at others arſenic acid. Being perſuaded that the formation and properties of this gas deſerved a cloſer examination, I inſtituted a ſeries of experiments, the reſults of which are as follows. Prouſt mentions it.

Methods of obtaining arſeniated Hidrogen Gas.

1. There are a variety of proceſſes for obtaining arſeniated hidrogen. It is produced by heating tin filings in liquid arſenic acid. This method is the moſt expenſive and moſt tedious. During the evolution of the gas in this proceſs, arſenic, alloyed with tin, is precipitated, and the fluid obtained, holds in ſolution, arſeniate of tin. Proceſſes for obtaining arſeniated hidrogen gas.

2. It is likewiſe formed by treating in a ſimilar manner, arſenic and iron with muriatic acid.

3. Arſeniated hidrogen is alſo produced by heating a mixture of arſenious acid, iron filings and muriatic acid. The fluid, in this caſe, contains muriate of iron and muriate of arſenic.

4. Tin filings and arſenic acid yield this gas under ſimilar circumſtances.

5. Four parts of granulated zinc and one of arſenic, treated in a ſimilar manner with ſulphuric acid, previouſly diluted with two parts of water, afford arſeniated hidrogen very readily.

The gas obtained according to either of theſe proceſſes, is nearly alike, but that produced according to the laſt proceſs ſeems to be the moſt perfect gas, for it contains no exceſs of hidrogen. When arſeniated hidrogen is produced by means of zinc, arſenic, and dilute of ſulphuric acid, the quantity of arſeniated hidrogen is leſs than the quantity of hidrogen which would be obtained in decompoſing water in a ſimilar manner, without the interpoſition of arſenic. The reſidue, after the evolution of the gas has ceaſed, contains metallic arſenic; part of the hidrogen muſt therefore have acted on the oxigen of the arſenic acid, in order to reduce it to the metallic ſtate. From what has been ſtated, it appears that arſeniated hidrogen contains arſenic in a metallic ſtate, and not in the ſtate of arſenious, or arſenic acid. This will become more obvious in the ſequel of this paper. Beſt produced from zinc, arſenic, and ſulphuric acid.

* Journ. de Phys. T. II. p. 173.

Phyſical

Phyſical Properties of Arſeniated Hidrogen Gas.

Its phyſical properties.

Arſeniated hidrogen is a permanent elaſtic aeriform inviſible fluid. It is a true chemical compound. Prouſt aſſerts that it depoſits arſenic: This however I have never been able to obſerve, if the gas were *pure*. It has an alliaceous fetid ſmell. It extinguiſhes burning bodies. It is not abſorbable by water; but when this fluid is freed from atmoſpheric air, it takes up a ſmall quantity of the gas which becomes diſengaged again by mere agitation. It does not change the colour of tincture of litmus. The ſpecific gravity of arſeniated hidrogen is, at 28′ barometrical preſſure = 0,5293, or, one cubic inch (old French meaſure) weighs 0,2435 grains. It is therefore lighter than oxigen, nitrogen, atmoſpheric air, carbonic acid, nitrous gas, ammonia, and gazeous oxide of carbon, but heavier than hidrogen and ſulphurated hidrogen gaſes. It is abſolutely fatal to animal life.

Chemical Properties of Arſeniated Hidrogen Gas.

Its chemical properties.

Arſeniated hidrogen, mingled with atmoſpheric air, ſuffers no chemical change, but mere dilution. The ſame holds good with reſpect to nitrogen. When mingled with nitrous gas, a diminution of 0,02, or 0,03 takes place, which ſometimes even amounts to 0,05. To aſcertain the nature of this gas, I mixed two parts of arſeniated hidrogen, with one of nitrous gas, and gradually added oxigen, till no further diminution of bulk enſued. On preſenting to this mixture a lighted taper, a loud exploſion took place, accompanied with flame. Probably part of the oxigen added, remained uncombined; for a mixture of two parts of nitrous gas, and three of arſeniated hidrogen, could not be inflamed by the taper; arſeniated hidrogen is miſceable with hidrogen, with carbonic acid, and with ammonia in all proportions.

Mixed with nitrous gas.

With oxiginized muriatic acid gas.

Into a cylinder half filled with arſenic and hidrogen, I ſent up bubbles of oxiginized muriatic acid gas. The bulk of the gas was diminiſhed, heat was evolved, and metallic arſenic was depoſited in a cryſtalline ſtate. On adding to the mixture an additional doſe of oxiginized muriatic acid gas, white fumes appeared, and the depoſited metal vaniſhed. The ſame experiment was repeated ſucceſſively, taking care to add no more of the latter gas, than was juſt ſufficient to occaſion the precipitation

cipitation of metallic arfenic. The collected metal yielded nitrous gas, by the affufion of nitric acid, and on adding to this mixture muriatic acid, arfenic acid was produced. The arfenic depofited in the manner ftated before, when laid on ignited coals, became volatilized in thick white fumes, yielding arfenious acid. The precipitation of metallic arfenic muft be afcribed to the decompofition of the oxiginized muriatic acid gas; the oxigen of this gas uniting with part of the hidrogen of the arfeniated hidrogen, and forming water, and thus feparating the arfenic. For the arfenic is capable of being oxided by the muriatic acid. Should it be imagined, that arfenic exifted in arfeniated hidrogen, in the oxidized ftate, and that it became precipitated by the oxiginized muriatic acid robbing it of its oxigen, we fuppofe things analogically erroneous, for the oxiginized muriatic acid is more capable of giving out oxigen than of taking it. The experiments of Chenevix feems perhaps hoftile to this affertion; but the experiments of this philofopher merely prove that the oxiginized muriatic acid is capable of combining with an additional dofe of oxigen, and conftituting with it a hyperoxiginized muriatic acid. This certainly cannot be the cafe in the prefent inftance, as will appear more evident from what I fhall ftate prefently.

Further experiments with this gas over quickfilver.

I filled a cylinder in the mercurial pneumatic trough, with arfeniated hidrogen, and fent up into it as expeditioufly as poffible, a quantity of oxiginized muriatic acid gas. The refult was evolution of heat, diminution of volume, and the inner fides of the cylinder became covered with a kind of dew. A formation of water had therefore actually taken place in this experiment. Into another dry cylinder half filled over mercury, with arfeniated hidrogen, I introduced dry muriatic acid gas. In this cafe no diminution of bulk, no feparation of arfenic enfued; no change at all took place. Repeating the fame experiment, I introduced into the cylinder a fmall quantity of water; the muriatic acid gas was abforbed, and the refidue was arfeniated hidrogen unaltered.

Into a cylinder half filled with oxiginized muriatic acid gas, I paffed gradually arfeniated hidrogen in fmall bubbles at a time; in this cafe no metallic arfenic was feparated, but thick white clouds appeared. On continuing the addition of arfeniated hidrogen till no more white fumes appeared, metallic arfenic was depofited. It follows from this experiment, that when

when a *small* quantity of arfeniated hidrogen is made to act upon a *large* quantity of oxiginized muriatic acid gas, part of the oxigen of the oxiginized muriatic acid gas combines, not only with the hidrogen of the arfeniated gas, and forms water, but the metallic arfenic alfo becomes oxidifed. Reafoning from this fact, we fhould be inclined to believe, that a mutual decompofition of both the gafes could be thus effected; but this cannot be accomplifhed; a diminution of bulk indeed takes place to a certain extent, but the complete difappearance of both the gafes cannot be effected. If the admixture of arfeniated hidrogen, with this oxiginized muriatic acid gas, be continued no longer than white clouds appear, and the refidue be then examined, it will be found to confift of hidrogen and oxiginized muriatic acid gafes; and the mixture detonates at the approach of a taper. The oxiginized muriatic acid gas can only be feparated with difficulty by long agitation, in contact with water, and it feems as if it were become lefs foluble in that fluid. If the feparation of this gas be accomplifhed, the remaining arfeniated hidrogen burns with a pure flame, void of alliaceous odour, and contains no veftige of arfenic, as fhall be proved hereafter. From what has been ftated, the following theory may be formed.

Theory of the decompofition of arfeniated hidrogen gas.

Arfenic, in combination with a certain portion of hidrogen, conftitutes arfeniated hidrogen gas. On prefenting to this combination oxiginized muriatic acid gas, the oxigen of this gas combines with the hidrogen, which held in folution the arfenic, and the latter is feparated. If more oxiginized muriatic acid be added than is neceffary for this purpofe, the portion of oxiginized muriatic acid gas does not act further upon the hidrogen, but merely upon the arfenic, and the latter becomes oxidifed.

Arfeniated hidrogen gas mixed with hidrogen.

Hidrogen and arfeniated hidrogen may be mingled without decompofing each other; the decompofition can only be effected by the contact of fire; but if we mingle hidrogen, holding in folution fulphur and oxiginized muriatic acid gas, the decompofition and formation of water is inftantly effected. This is likewife the cafe with arfeniated hidrogen gas.

Hidrogen combined with metallic fubftances.

Hitherto no combination of hidrogen with a metallic fubftance has been known; but it is highly probable, that fuch combinations may exift. This indeed feems to be the cafe in the formation of this gas on which we are treating. If this be ad-

mitted, a divifion of hidrogen muft take place, in the following manner; one part of it muft unite with the oxigen of the oxiginized muriatic acid gas, to produce water; another part muft fall down with the arfenic; and another portion remains combined with caloric, in the form of hidrogen gas.

Hydrothian acid gas * and arfeniated hidrogen do not act upon each other. To a mixture of equal parts of hydrothian acid gas, and arfeniated hidrogen gas, I added gradually oxiginized muriatic acid gas; a diminution of volume inftantly took place, accompanied with liberation of heat, and a depofition of yellow fulphurized arfenic (orpiment). On adding an additional quantity of gas, the precipitate acquired a beautiful orange red colour, and on continuing the addition of oxiginized muriatic acid gas, white clouds were produced, the precipitate detached itfelf from the fides of the veffel, and were gradually converted into a pulverulent fubftance of a yellowifh white colour. Sulphurated and arfeniated hidrogen gafes. Oxiginized muriatic acid gas added to thefe.

The refults of thefe experiments are obvious, and might have been expected *a priori*. But they may ferve as a teft to difcover the prefence of arfeniated hidrogen, when contained in other gafes. Teft of arfeniated hidrogen.

I mingled one cubic inch of arfeniated hidrogen with ten of nitrogen, and one of hydrothian acid (fulphurated hidrogen gas;) on adding to this mixture a fmall quantity of oxiginized muriatic acid gas, yellow fulphurized arfenic was inftantly depofited. It is not improbable, that arfenic is likewife foluble in other gafes, and in this cafe the hydrothian acid (liquid fulphurated hidrogen,) in conjunction with oxiginized muriatic acid, would prove a ufeful re-agent for difcovering the prefence of it. Nitrogen mixed with it.

A lighted taper immerfed in a vial filled with arfeniated hidrogen, is inftantly extinguifhed; at the fame time that the gas burns at the orifice of the vial with a lambent white flame, diffufing a difagreeable odour, and much white fumes, which are arfenious acid. If the gas be inflamed in a phial with a fmall orifice, the flame defcends gradually down to the bottom of the phial, which becomes lined with a coat of cryftallized metallic arfenic. In this cafe therefore the hidrogen alone burns. Extinguifhes a lighted candle, but inflames.

* The name given by the Germans to fulphurated hidrogen gas, on account of its poffeffing the properties of an acid.

If

Explodes with oxigen.

If two parts of arfeniated hidrogen be mingled with three of oxigen, and a taper be prefented to the mixture, an explofion takes place; no metallic arfenic is feparated, but the products are arfenious acid and water: foap-bubbles with the mixture of thefe gafes, explode with a bluifh white flame, leaving a white fmoke and ftrong alliaceous odour. Equal parts of arfeniated hidrogen and oxigen gafes, fired in like manner, do not explode fo loudly, but the report is accompanied with a much more vivid flame. A ftream of arfeniated hidrogen, iffuing from a bladder fitted with a ftop-cock, and fet to burn in a large receiver filled with oxigen, yielded arfenic acid. The combuftion in this manner is uncommonly beautiful; the gas burns with a blue flame of uncommon fplendor.

Burns beautifully in oxigen gas.

Two parts of arfeniated hidrogen, and one of oxigen gas, being detonated in a clofe veffel by means of the electric fpark, left a fmall refiduum; on repeating the experiment, the detonating tube broke during the explofion, which prevented the examination of the refidue. From what has been fo far related, it becomes evident that the conftituent parts of arfeniated hidrogen gas, are metallic arfenic and hidrogen. Were it poffible to determine with abfolute certainty, that no increafe of volume took place during the folution of arfenic in hidrogen, the proportion of the conftituent parts of this gas might be afcertained thus:

Compofed of metallic arfenic and hidrogen.

	French weight and meafure.
One cubic inch of hidrogen, weighs -	0,0353
One cubic inch of arfeniated hidrogen, weighs	0,2435
Deducting the former from the latter, we get	0,2082

Which is the quantity of arfenic diffolved in the gas, confequently one cubic inch of arfeniated hidrogen gas, confifts of 0,0363 hidrogen, and 0,2082 arfenic; and one cubic inch of this gas contains about $\frac{1}{4}$ grain of metallic arfenic.

Habitudes of arfeniated Hidrogen to Acids.

Its habitudes to acids. Concentrated nitric acid.

Into a phial, containing about eight cubic inches of arfeniated hidrogen, I poured a half cubic inch of concentrated nitric acid. The moment the acid came into contact with the gas

gas. The phial was filled with denfe red fumes, a white flame pervaded the veffel, and a detonation enfued.

Dilute nitric acid.

On repeating the experiment with dilute nitric acid, no accenfion took place. The refiduary gas was pure hidrogen, and the water contained arfenic acid. Fuming concentrated nitrous acid therefore is capable of oxidifing the arfenic contained in this gas, at the fame time that the oxigen of the acid burns with the hidrogen of the gas, and produces water; whereas weak nitric acid is only capable of oxidizing the arfenic, without acting upon the hidrogen prefent.

Nitro-muriatic acid.

Into a glafs tube, furnifhed with a ftopper at one extremity, and clofed at the other, I introduced eight cubic inches of arfeniated hidrogen, to which were added two cubic inches of nitro-muriatic acid. After having agitated the fluids, on opening the tube under water, a diminution of one cubic inch took place. The refiduary gas was pure hidrogen. It is remarkable that, during the addition of the nitro-muriatic acid, a black powder feparated, which again difappeared on agitating the tube. Nitro-muriatic acid acts therefore in the fame manner upon this gas, as oxigenized muriatic acid gas. It effects firft a feparation of the metallic arfenic, and then oxiginizes this metal. Liquid exigenized muriatic acid decompofes arfeniated hidrogen by mere agitation; the refidue is hidrogen. Muriatic acid exercifes very little action upon arfeniated hidrogen; but merely diffolves a minute portion of it, which may be expelled again by heat. Concentrated acetic acid has no effect upon it.

Oxiginized muriatic acid.

Muriatic acid.

Concentrated acetic acid.

Concentrated fulphuric acid.

Into a glafs cylinder holding eight cubic inches of arfeniated hidrogen, I poured one cubic inch of concentrated fulphuric acid, and then clofed the tube. At the moment of the addition of the acid, the cylinder became lined with a coat of bright metallic arfenic, fo as to refemble a looking-glafs. On agitating the cylinder, the coating refolved itfelf into a brownifh black powder, which, after a few days, affumed the colour of Kermes mineral. On opening the cylinder under water, a diminution of bulk enfued, and the refiduary gas proved to be hidrogen. The experiment was repeated, and yielded the fame refults. The fulphuric acid employed in this experiment, had acquired a penetrating pungent fmell, and was examined, after having been neutralized by ammonia, in the following manner:

Curious phenomenon.

The fulphuric acid examined.

5 Ammo-

Ammoniate of copper, on being mingled with it, acquired a greenish colour. Hydrosulphuret of ammonia instantly occasioned a copious yellow precipitate.

Water impregnated with sulphurated hidrogen gas, occasioned a similar effect. From the results of these tests it becomes obvious, that the acid consisted of sulphuric, sulphureous and arsenic acid. In order to be certain in this respect, I mingled a few drops of liquid arsenic acid with a mixture of sulphuric and sulphureous acid, neutralized the fluid with ammonia, and submitted it to the same tests. The results of this mixture were analogous to the former. The decomposition of the arseniated hidrogen gas, is probably analagous to the decomposition of this gas, by means of oxiginized muriatic acid gas. The sulphuric acid first gives up part of its oxigen to the hidrogen of the arseniated gas, and occasions the separation of the arsenic; which, at the expence of the remaining portion of oxigen of the sulphuric acid, becomes afterwards oxigenized, and constitutes the arsenic acid.

Theory.

Its habitudes to metallic solutions.

Habitudes of arseniated Hydrogen Gas to Metallic Solutions.

Ammoniate of copper.

I caused a current of arseniated hidrogen gas, to pass through a solution of ammoniate of copper. A metallic pellicle appeared on the surface of the fluid, which suffered no other change.

Muriate of tin.

Into a bottle filled with arseniated hidrogen gas, I dropped a solution of muriate of tin. On agitating the solution, it acquired a brown colour, a partial diminution of the gas ensued, but the solution of tin was not converted into an oxidized muriate of tin, which would have been the case, if the arsenic existed in the gas in an oxidized state.

Nitrate of lead.

Nitrate of lead, on being brought into contact with arseniated hidrogen gas, became turbid, and deposited a precipitate, which was arseniate of lead.

Nitrate of silver.

Nitrate of silver submitted to the action of the gas, became instantly of an intense black, and a pellicle of metallic silver collected on the surface of the fluid. The residue of the gas, which had been made to act on the oxide of silver for some time, had all the properties of pure hidrogen.

A good test.

This experiment shows, that nitrate of silver might be employed for detecting the presence of arseniated hidrogen; for

as

as long as a minute quantity of arſenic was preſent, a black precipitate enſued, whereas *pure* hidrogen has no effect upon this re-agent.

I paſſed into a concentrated ſolution of nitrate of ſilver, a ſtream of arſeniated hidrogen, collected the black metallic precipitate, waſhed and dried it. The fluid obtained in this proceſs did not diſturb the tranſparency, or change the colour of ammoniate of copper. Neither liquid ſulphurated hidrogen, tincture of galls, nor potaſh, had any effect upon it. It contained therefore neither ſilver nor arſenic. The precipitate before obtained, acquired a metallic luſtre on being ſaturated; laid on ignited coals, it diffuſed an odour of arſenic, and it yielded by fuſion a button of ſilver. It was an arſeniate of ſilver.

Arſeniated hidrogen paſſed into a ſolution of nitro-muriate of gold, occaſioned a precipitate; on the ſurface of the fluid appeared a pellicle of metallic gold; and the ſides of the veſſel, in contact with the fluid, became beautifully gilded. The fluid through which the gas had been paſſed, examined in the uſual manner, proved to contain no veſtige either of gold or arſenic. The precipitate greatly reſembled charcoal duſt, interſperſed with minute particles of gold. Nitro-muriate of gold.

It is highly probable, that arſeniated hidrogen is capable of decompoſing all metallic ſolutions, the baſis of which is either nitric, or muriatic acid, and probably other acids. Decompoſes metallic ſolutions.

Habitudes of arſeniated Hidrogen Gas to various other Bodies.

Expreſſed oils, on being agitated for ſome time in contact with arſeniated hidrogen, abſorbed part of the gas, and acquired a deeper colour. Its habitudes to expreſſed oils,

Alcohol ſuffers no change from arſeniated hidrogen. Solution of potaſh, and liquid ammonia, do not abſorb it. and to alcohol, and alcalies.

Such are the properties of this gas, the inveſtigation of which I ſhall continue as ſoon as my health is reſtored, it being ſo conſiderably injured by the unavoidable inhalation of this gas during the courſe of theſe experiments, which gives me ample reaſon to conclude, that the gas muſt be highly poiſonous.

D. J. B. TROMSDORFF.

Erfurth, Feb. 1803.

VIII.

Account of an Eudiometric Apparatus, contrived and used by Dr. HOPE, *Professor of Chemistry in the University of Edinburgh**.

Dr. Hope's eudiometric apparatus.

SINCE the discovery of the uncertainty with which the application of nitrous gas to atmospheric air, and other mixtures, containing oxigen is attended, it has been found desirable to present solid or liquid substances for the absorption of that principle. This, on first consideration, may seem at least as easy to be done, as to mix two gases; but it is by no means so, because the liquids in particular possess a degree of chemical activity, which renders it inconvenient to immerse the hands in them, or to expose their surfaces to the open air, especially when it is attempted to accelerate their operation by means of agitation, so as to obviate the principal objection to their use, the tardiness of the process.

The apparatus of Dr. Hope, which he uses in his lectures and in his experiments, is at once simple and effectual, and I have the pleasure of inserting the following correct description with his permission.

The apparatus consists of two bottles, which are represented in *Plate* XII, connected together in the manner in which they are used; A represents a small bottle which may be nearly two inches in external diameter, and three in length, having a neck and stopper at D, and another neck as usual at C. It is destined to contain the eudiometric liquor. B represents a larger bottle, which may be nearly of the same diameter, or rather of somewhat less, but $8\frac{1}{2}$ inches long. The neck of B is fitted accurately by grinding into the neck of A at C.

The method of using this apparatus is very simple: introduce in the ordinary way into the bottle B, the air or gas

* As the description at p. 61, of the present volume is in some respects inaccurate through haste, and the figure, being an outline, appears as if the neck of the upper vessel protruded so far into the lower, as to prevent the ascent of a portion of the gas after agitation,—I have chosen rather to give an entire description and drawing in this place, than adopt the less acceptable process of annotating and correcting.

till

till it is full; then fill A with the abforbing liquor, for example with a folution of fulphuret of lime, which Dr. Hope commonly employs, and covering the mouth with a flat piece of glafs, plunge it under the furface of water, and there infert the neck of B. Dr. Hope's eudiometric apparatus.

The compound veffel is then removed from the water, and inclined till a fufficient quantity of the liquor flows into B. It is now well fhaken, and the agitation ought to be continued till the abforption is compleated—Left the diminution of the denfity of the included elaftic fluid fhould retard the abforption of the oxigenous portion; from time to time the apparatus, in the pofition in which it is reprefented in the figure, is to be placed in a plate full of water, and the ftopper D is to be loofened, or fo far withdrawn, as to allow this fluid to enter to fill the place of the abforbed gas.—By this admixture of water the liquor is diluted, but not to fuch a degree as in any meafure to interrupt the advancement of the procefs, unlefs indeed when the gas abounds very much in oxigen.

When a gas of this defcription is the fubject of experiment, it may be proper to ufe an apparatus, of which the bottle A is made of greater capacity in relation to the fize of B, than in the proportion already affigned.

As foon as it is obferved, that after reiterated agitation, and opening the ftopper D, the liquor does not rife higher, the abforption may be confidered as compleated, and the operation may be finifhed by allowing the inftrument to regain its original temperature, in cafe, from want of due precaution, it may have been affected in this refpect by the warmth of the hand in the courfe of the experiments.

If the bottle B be graduated, the amount of the abforption may be determined at once, by plunging the apparatus into water to the level of the included liquid, and removing the ftopper, otherwife the refidual gas may be transferred into a tube, exprefsly graduated for meafuring gafes.

By this convenient contrivance, we fee that the liquid is economized and the celerity, neatnefs, and precifion of experiment are enfured. The fize here mentioned is very well adapted to the purpofes of publie exhibition, but it is almoft needlefs to remark, that it may, and in general ought to be made confiderably fmaller for the ordinary eudiometric experiments.

Dr. Hope ſuggeſted that the apparatus might be made ſtill more ſimple without impairing its merits, in any conſiderable degree, particularly when ſmall volumes of gas are to be examined. This is done by uſing a ſmall bottle having one neck only, and having a graduated tube nine or ten inches long, and from half to three quarters of an inch in diameter, accurately adjuſted to fit into it, but not projecting into its cavity.

If the bottle have twice or thrice the capacity of the tube, the ſame ſolution of ſulphuret of lime may be repeatedly uſed, and the abſorption will be more expeditious.

In employing this inſtrument, the manipulation is in all reſpects the ſame as above deſcribed, excepting when the progreſs or termination of the operation is to be diſcovered. For this purpoſe, looſen the connection between the tube and the bottle, in a degree ſufficient to allow the ingreſs of the water of the trough, in which the eudiometer muſt then be immerſed.

This apparatus equally unites diſpatch, œconomy of eudiometric liquor, and convenience of management.

IX.

Deſcription of an Apparatus for drying the Products of Chemical Analyſis which is alſo uſeful for Experiments of Congelation. By Mr. FREDERICK ACCUM. *Communicated by the Inventor.*

Apparatus for drying precipitates on the water bath:

THIS apparatus, Plate X. is extremely uſeful in drying ſuch products as abſolutely require a temperature not exceeding 212°; ſuch as fulminating mercury, Chenevix's fulminating ſilver, and other exploſive compounds. The ſubſtance to be dried muſt be placed in the conical glaſs veſſel B, and when the veſſel E is filled with water up to the ſide tube D; the deſiccation may be performed without any riſk of exploſion, or any further trouble, by putting the apparatus over a lamp, and keeping the water in a ſtate of ebullition. *(See the lower drawing in perſpective.)* I have found it particularly uſeful in the deſiccation of the precipitates obtained in the analyſis of minerals. It is well known that the ſame mineral, analyſed

analyfed by different chemifts, has been found to yield different proportions of the fame ingrediants, and that the difference of proportions of the conftituent parts, in many cafes, is often more apparent, than real; arifing entirely from the various degrees of deficcation that has been employed by different analyfts, and fometimes even by the fame perfon. This point is of fuch importance, and is productive of fo much trouble, that every chemift who has analyzed a mineral water, or cryftallized and feparated fmall quantities of deliquefcent falts, will at once perceive the utility of the apparatus in this refpect.

This apparatus may likewife be ufed as a *water-bath*. In that cafe, the conical glafs veffel B is removed, and the inner tin veffel E filled with water; into this, retorts, flafks, gallipots, vials, bottles, &c. may be immerfed for promoting the proceffes of diftillation, digeftion, folution, evaporation, &c. or it may be ufed as a *fand bath*, (it being hard foldered) by filling the tin veffel with fifted fand, for performing thofe operations which require a higher temperature, than that of boiling water.

The fame ufed as a water bath:

or fand bath.

When the inftrument is required to be ufed as a *freezing apparatus*, the bottom cover G is to be taken off, and the cavity between the interior, and exterior veffel, filled with the frigorific mixture; a wetted piece of bladder is then to be tied over the opening, or the cover is put on, to retain the mixture. The fecond frigorific material (for inftance if quickfilver is to be frozen) confifting we will fay, of muriate of lime and fnow, are to be cooled by the mixture in the exterior veffel, by putting the muriate of lime into the conical glafs veffel, together with the mercury contained in a thin glafs tube; and furrounding the glafs veffel, by filling the interior tin veffel with fnow, or pulverized ice. When thefe materials have been cooled down to 0°; the fnow and ice may be mixed together by emptying the muriate of lime into the veffel containing the fnow, and ftirring the mixture with a glafs rod to facilitate the folution of the falt, and to produce the requifite degree of cold. The number of apparatus I have fold to philofophical chemifts, gives me reafon to fuppofe, that they have proved ufeful.

Method of freezing (mercury for example) by this inftrument.

FREDERICK ACCUM.

11, Old Compton Street, Soho.

Letter

X.

Letter from Mr. ACCUM, *in anſwer to the Enquiries of a Correſpondent reſpecting the Proceſs for obtaining the Aguſtine Earth.*

To Mr. NICHOLSON,

DEAR SIR,

Miſtake reſpecting the publication of the method of obtaining aguſtine earth.

YOUR correſpondent, P. O. in the laſt number of your Journal, is correct, when he obſerves, that the proceſs for obtaining aguſtine earth is not noticed in my ſyſtem of practical chemiſtry, nor in any other work publiſhed in this country, and alſo that moſt of the books he quotes, were publiſhed a conſiderable time after this earth *was made known* by profeſſor Tromsdorf; but he is miſtaken in his opinion, that it was known in this country previous to the publication of the above works,

Fiſt account.

The method of ſeparating this earth from the mineral which contains it, had not then been communicated to us through the uſual channels of ſcientific information. The firſt account of the method of ſeparating this earth I can find, is contained in a german work, entitled *Practiſche Anleitung zur zerlegenden Chiemie,* publiſhed by Profeſſor Göetling, 1802. From which the following tranſlation is made.

The proceſs. Saxon beril pounded; boiled with potaſh; then fuſed;

Let a determinate quantity of the Saxon beril finely levigated, be boiled in a ſilver veſſel, with three or four times its weight of potaſh, diſſolved in a ſufficient quantity of water; evaporate the whole to dryneſs, and fuſe the maſs.

ſoftened by water; diſſolved in muriatic acid; evaporated to dryneſs; diluted with water; and the ſilex ſeparated;

Soften the alcaline maſs by the gradual addition of water, and when detached from the crucible, add to it muriatic acid till the whole is diſſolved. Evaporate the ſolution to dryneſs, boil the maſs in a ſufficient quantity of water, and ſeparate the inſoluble reſidue [ſilex] by the filtre.

the fluid precipitated by carbonate of ſoda;

Decompoſe the fluid from which the ſiliceous earth has been ſeparated, by gradually mingling it with a ſolution of carbonate of ſoda; collect the precipitate, and waſh it repeatedly.

ſeparate the alumine from the precipitate by potaſh which diſſolves it and leaves the *aguſtine earth.*

When the precipitate obtained in the laſt proceſs has acquired ſome conſiſtence, transfer it into a flaſk containing a concentrated ſolution of potaſh. The alumine which was preſent

present in the mineral, will be diſſolved*, and the inſoluble reſidue left, is the new earth called *Aguſtine*.

It is diſtinguiſhed from all other earths by being abſolutely inſoluble in potaſh, ſoda, and ammonia, and all their carbonates. Nor can an union of either of the two firſt alcalies with Aguſtine be effected by fuſion. It is ſoluble in acids, with which it forms ſalts, which have little or no taſte. It is ſoluble in acids with equal facility after having been ignited, as when freſh prepared. It fuſes with borax into a tranſparent colourleſs glaſs. Characters of this earth.

100 parts of the Saxon beril yielded Profeſſor Tromſdorf 78,0 Aguſtine, 4,5 alumine, and 15,0 ſilex.

I am, Sir,

Your moſt obedient,

FREDERICK ACCUM.

11, *Old Compton Street, Soho,*
15*th October*, 1803.

XI.

Letter from a Correſpondent concerning the Method propoſed by Mr. Carliſle for cloſing wide-mouthed Veſſels.

October 15, 1803.

SIR,

IN the laſt number of your Journal, publiſhed on the firſt of this month, page 68, I find a letter addreſſed to you by Mr. Carliſle, deſcribing " a method of cloſing wide mouthed veſſels intended to be kept from communicating with the air ;" and in *Plate* V. *Fig*. 2, there is an engraving of the veſſel re- The method of cloſing veſſels ſuggeſted in our laſt number

* It is perhaps needleſs to ſtate that the alumine may be ſeparated from the alcaline ſolution, by ſaturating it with muriatic acid in exceſs, ſo as to neutralize not only all the potaſh, but alſo to diſſolve the alumine, and then to decompoſe the obtained ſolution by carbonate of ammonia. Should glucine be expected, the carbonate of ammonia ſhould be added in conſiderable exceſs; for the exceſs of ammonia retains in ſolution the glucine, and nothing but alumine will be thrown down. The glucine may be obtained by evaporating the ſolution to dryneſs and igniting the reſidue.

F. A.

com-

commended by the above ingenious gentleman. But I muſt beg leave to obſerve that this, which in your Journal is denominated "a new method, by a jar, the cover of which fits into a groove with hog's lard," is in reality a very old method; for in the quarto edition of Buffon, publiſhed at Paris in 1749, Tome III. p. 192, you will find many ſuch veſſels repreſented, and *Fig.* 4, is preciſely the ſame as the one given in your work.

Was uſed by Buffon;

But Buffon was not the inventor of this method, for it is claimed by the celebrated Mr. Le Cat, who had glaſſes made upon this conſtruction about the year 1739, and in 1748 ſent to the Royal Society a deſcription, which, with an engraving, may be ſeen in the 46th volume of the Philoſophical Tranſactions, page 6.

and by Le Cat;

But even the celebrated Mr. Le Cat was not the inventor, for old Glauber employed this method long before, and a plate and deſcription may be ſeen by any one who will take the trouble to conſult his "*Fornacum Philoſophicarum, pars quinta,*" page 13, &c. &c. publiſhed at Amſterdam in 1661. The only difference is, that Glauber uſed quickſilver to fill the groove; Mr. Le Cat employed quickſilver or oil; and Mr. Carliſle recommends hog's lard. If you think theſe remarks worthy of a place in the next number of your Journal, they are much at your ſervice; from an

nd before all by Glauber.

OLD CORRESPONDENT.

XII.

Account of an Experiment for ſupplying Worm Tubs and other Refrigeratories by the aſſiſtant Preſſure of the Atmoſphere, which proved unſucceſsful, on a large Scale; to which is added an Improvement for extending the uſeful Applications of the Syphon. By EDWARD HOWARD, ESQ. F. R. S. *In a Letter to the Editor.*

To Mr. NICHOLSON.

SIR,

THE method of ſupplying worm tubs and condenſers, given by Sir Alexander Edelcrantz in your laſt number, induces me to trouble you with the reſult of an unſucceſsful experiment, which I ſome time ſince made on much the ſame principle, and

Apparatus for more eaſily raiſing water in worm tubs, by atmoſpheric preſſure.

and on a worm tub of a confiderable fize. Indeed it was my firft intention to have made the worm tub part of the arm of a fyphon; but, as warm water was conftantly wanted in an elevated part of the premifes, and as all the water ufed was raifed by a lifting pump from an adjacent well, an opportunity appeared to prefent itfelf, both of economizing labour, and of making ufe of the warm water of the upper furface of the worm tub. To effect thefe objects, I made the pipe coming from the well immediately communicate with the tub, and annexed the pump, by means of another pipe, to the upper furface of the tub. By this conftruction, there was every reafon to conclude that it would be practicable to pump off the warm water, and alfo, that whenever the pump was worked to fupply the other demands of the laboratory, the water in the worm tub would be conftantly changed without additional labour. It was further thought neceffary to add a valve of fafety to the upper part of the tub, and two ftop cocks, one on the pipe leading from the well to the tub, and the other on the pipe leading from the pump to the tub, in order that by the regulation of thefe cocks, water might be had either immediately from the well, for other purpofes of condenfation, &c. or it might be drawn from the worm tub for proceffes requiring warm water, or for ufes to which warm or cold water might be indifferently applied.

To give a better idea of the apparatus, I have fubjoined the following outline, *Plate* XII. where A reprefents the worm tub; B a pipe leading from the well; C a pipe communicating immediately from the pump; D a valve of fafety, E a pump; F a well; and G G ftop cocks.

The apparatus when made upon a fmall fcale, with a Woulfe's bottle and glafs tubes, anfwered perfectly and promifed to be a valuable acquifition. I wifh I could relate the fuccefs of the fame experiment made upon a large one: But, notwithftanding the beft workmen in London were employed, they could not make the joints of the worm tub fufficiently tight to refift the preffure of the atmofphere for more than a few fucceffive hours.

The apparatus fucceeded perfectly in the fmall way; but failed on a large fcale.

I fhould not, Sir, have offered to you the refult of an unfuccefsful experiment were it not from an apprehenfion that the worm tub recommended by Sir Alexander Edelcrantz might be no lefs difficult to conftruct than the one I have defcribed.

Allow

Improvement in the fyphon.

Allow me, Sir, to take advantage of this opportunity to communicate what I believe to be an improvement of the fyphon. It was made at the time I had an idea of applying the principles of a fyphon to the worm tub: Although it may not be applicable to this purpofe, it will be found exceedingly ufeful in cafes where it would be difagreeable, dangerous, or impoffible, to exhauft the fyphon by the common mode of fucking out the air, if I may be allowed fuch an expreffion. I think it even probable that fyphons of confiderable dimenfions may be introduced for emptying ponds, or for lowering the water in mill-dams or canals; for it is without doubt defirable to avoid cutting through an embankment, and a very large inftrument of the new conftruction would be thus put in action with great eafe. The improvement, Sir, I have thus ventured to fpeak of confifts merely in enlarging the exhaufting pipe to the fame calibre as the reft of the fyphon; in elevating it a little, and in opening its mouth like a funnel. See *Fig.* 3. *Plate* XI.

It is fcarcely neceffary to point out that, to ufe fuch a fyphon, the fhort arm is, as ufual, to be immerfed in the liquor intended to be operated upon, and the aperture of the long arm to be clofed whilft the whole inftrument is to be filled through the funnel with fome of the fame liquor.

I am,

SIR,

Your obedient humble fervant,

EDWARD HOWARD.

XIII.

A Method of equalizing the Motion of a Steam Engine without the Affiftance of a Fly Wheel. By Mr. ARTHUR WOOLF, *Engineer. Communicated by the Inventor.*

Equalizing mechanifm which allows the engine to fet off or ftop in any part of the ftroke.

THE mechanifm here prefented as a fubftitute for the fly, poffeffes the advantage of equalizing the motion, with the power of being ftopped and fet to work at any part of the ftroke, the utility of which, in mines, collieries, and other works, will be immediately feen by thofe converfant in fuch undertakings.

Plate

Plate XI. *Fig*. 1. A reprefents part of the engine beam; B the connecting rod; C the crank arm; D a cog-wheel, working into another cog-wheel F, of half the fize; F a crank arm on the fhaft of the fmall wheel; G a cylinder clofed at bottom, in which a folid or unperforated pifton moves, leaving a vacuum beneath. This acts fimply inftead as a weight on the crank F, by the conftant preffure of the atmofphere; and the diameter of the pifton muft be fuch as nearly to equal one third of the power of the engine.

In *Fig*. 2. the outer circle is the line defcribed by the crank; the circumference of the inner circle is equal to twice the diameter of the outer, and the fquare has the fame circumference; this laft exhibits the inequality ftill remaining, which by this method is reduced to about one fifth; but by the affiftance of a fmall fly on the fecond motion, the effect will become nearly the fame as that of a rotative engine, with the advantages here mentioned.

The fame motion may be applied to a pump, but in this cafe the two cranks muft be horizontal at the fame time.

XIV.

Improvement by which the additional Arc in Mr. Ezekiel Walker's reflecting Quadrant is rendered unneceffary. In a Letter from the Inventor.

To Mr. NICHOLSON,

SIR,

Any one who has a juft idea of the reflecting quadrant, defcribed on page 218 of the fourth volume of your Journal, will perceive that one half of the arc of that inftrument is appropriated folely to the rectifying of the fecond horizon glafs. This method of adjufting is as good as any other that I have to propofe, but it is attended with the inconvenience of adding to the fize of the inftrument, and confequently to its weight.

Improvements in Mr. E. Walker's reflecting quadrant.

The following method of adjufting the fecond horizon glafs will reduce the inftrument to an octant, which will ftill poffefs the fame property of meafuring any angle lefs than 120° by the fore obfervation.

First

Improvement in Mr. E. Walker's reflecting quadrant.

Firſt, let a ſmall mirror be fixed upon the top of the index glaſs. This rectifier muſt be placed parallel to the firſt horizon glaſs, when the index ſtands at 90° on the arc, conſequently the index glaſs and rectifier will form an angle of 45 degrees. Secondly, let the two horizon glaſſes ſtand as high above the plane of the octant as the rectifier, and the inſtrument is ready for uſe.

After the firſt horizon glaſs has been adjuſted, bring the index to 90, and the rectifier will be parallel to the horizon glaſs, if the index glaſs and rectifier form an exact angle of 90° on the arc; but if they do not form that angle, the index will ſhow the error. Then to determine the error of the ſecond horizon glaſs, let the index be brought to 0, and the rectifier will in that ſituation perform the ſame office as the index glaſs in the quadrant, when the index ſtands at 90 at N; but as this has been fully explained in the deſcription of that inſtrument, it need not be further inſiſted on here.

An octant of five inches radius, conſtructed on theſe principles, would be exceedingly portable, and ſo ſtrong as not to be eaſily deranged by carriage; and theſe are properties which may recommend it to the attention of the traveller by land, particularly if he viſit thoſe latitudes, where the altitude of the ſun ſometimes exceeds 60 degrees.

Theſe two glaſſes may alſo be added to the ſextant, without depriving it of any of the valuable properties which it now poſſeſſes: and this additional apparatus need only be uſed in taking ſuch angular diſtances as are beyond the power of that inſtrument.

I am,

SIR,

Your's reſpectfully,

E. WALKER.

Lynn Regis, October 15*th*. 1803.

Plate XI. *Fig*. 3. A B repreſents the index glaſs; C D the rectifier; *m* the horizon glaſs; *n* the ſecond horizon glaſs.

SCIENTIFIC NEWS.

Abstract of Cit. SEGUIN's *Inquiries concerning Fermentation* *.

IN his firft paper Cit. Seguin explains the plan of inquiry he has undertaken concerning fermentation in general; and more particularly concerning the making of beer, wine, cider, malt and melaffes fpirits, &c. In his fecond his object is to prove, that fermentation is not produced by a fubftance *fui generis*, but by a combination of circumftances.

Fermentation not produced by a peculiar fubftance, but by a combination of circumftances.

He fhows, that, in the cafe of clear liquors fermenting, the true folvent of the fermentefcible caufe, whatever it be, is water, and not the faccharine matter; that the continuance of contact, and the prefence of fugar, are by no means neceffary for the folution of any fermentefcible principle in the yeaft; that this folution is made by water in a very fmall quantity, it is true, but almoft fuddenly, and even in the ordinary temperature of the air; finally, fuppofing fugar alfo to poffefs the property of diffolving any given fermentefcible principle, it would be impoffible to demonftrate this, fince, to render it perceptible, the fugar muft previoufly be diffolved in water.

Water the true folvent of the fermentative caufe, and thisfpeedily, and without the prefence of fugar being neceffary.

Additional Experiments of Mr. RITTER, *of Jena, on Galvanic Phenomena* †.

NATURAL philofophers will learn with pleafure, that Mr. Ritter continues his elegant experiments on a fubject, to the progrefs of which he has fo greatly contributed. As they all relate to a known theory, we fhall content ourfelves with giving the refults as communicated to us by Mr. Orfted, leaving to the experamentalift the tafk of proving them with all the requifite minutenefs.

The object of Mr. Ritter being to compare the electricity of Voltas' pile with that of electrical machines, he confiders fucceffively the intenfity of electricity, chemical action, fpark, and fhock, in the pile.

Mr. R's object to compare Volta's pile with electrical machines.

* Bulletin des Sciences, No. 75:
† Bulletin des Sciences, No. 77.

As

Electricity positive at one pole of the pile, negative at the other, diminishes between them, null in the centre. The time necessary for charging a battery, an inaccurate measure of intensity.

As to the intensity, we know that the electricity is positive at one of the poles of the pile, and negative at the other; it has been shown likewise, that it diminishes between these two extremes, so as to be null in the middle of the pile. Mr. Ritter sought to compare the degrees of intensity at these two poles, and those of different piles. This he attempted by determining the time necessary for charging a given battery; but this method is inaccurate, and no certainty can be attained in this respect, but by means of the electrical balance.

+ Electricity of the pile disposes metals to combine with oxigen, — with hidrogen. + pole armed with gold leaf, — with charcoal, on forming a communication the gold leaf is burned; if the charcoal be on the + side, it burns, and the gold is melted. — pole, brought into contact with quicksilver leaves a mark on its surface different from that of the +.

According to Mr. Ritter, the action of the positive pole of the pile disposes metals to combine with oxigen, and that of the negative pole disposes them to combine with hydrogen. If the positive pole be armed with a gold leaf, and the negative with a bit of charcoal, on forming a communication between these two substances the leaf of gold burns with a brilliant light, and the charcoal remains untouched: but if the charcoal be placed on the positive side, and the gold on the negative, the charcoal burns, and the gold is melted. If the negative pole be brought into contact with the shining surface of quicksilver, it leaves a trace different from that produced by the positive pole.

The effects of the pile on the animal body all reducible to expansions and contractions. The + pole increasing the bulk of parts, the — diminishing it. Effects on the tongue, and on the pulse. The expansion occasions a sense of heat, and *vice versa*.

Mr. Ritter asserts, that all the effects of the pile on the animal body are reducible to expansions and contractions. All the parts of the human body assume an increased bulk on the contact of the positive pole, and contract on the contact of the negative: for instance, the action of the positive pole on the tongue produces there, at the expiration of a few minutes, a slight elevation, whereas the negative pole occasions a little depression. If the same person touch the two poles with the two hands wetted, the intensity of the pulse is increased in the hand in contact with the positive pole, while its strength is diminished in the other, but the number of pulsations continues the same in each. The expansion thus produced in the organs, is attended with a sensation of heat, the contraction with a sense of cold.

To the eye the

If the eye be made to communicate with the positive pole, it sees objects red, larger, and more distinct; in contact with the negative pole it sees them blue, smaller, and more confused. The tongue receives from the positive pole an acid taste, from the negative an alcaline. The ear being in con-

tact

tact with the former, all sounds seem more grave: with the latter, more acute.

In general the two poles of the pile produce opposite effects.

Such are the results of Mr. Ritter's experiments. We have no opportunity of verifying their accuracy; but their singularity, their number, and particularly the ingenuity of their author, lead us to presume, that this account of them will be read with pleasure.

SOCIETE PHILOMATH.

+ pole makes objects red, large & distinct; the —, blue, small, and confused.
+ gives an acid taste,— alcaline.
+ deepens sounds.
— lightens them.
The two poles generally produce opposite effects.

Abstract of some Remarks on the Acetite of Lead, by Cit. THENARD *.

A MANUFACTURER of acetite of lead was obliged to stop his works, being no longer able to make the salt crystallize in needles, but always obtaining it in laminæ, which induced purchasers to refuse it; Cit. Thenard inquired into the cause of this phenomenon, and soon perceived, that it was owing to the proportions of the constituent principles of the salt. He succeeded in forming a salt perfectly similar to it, by boiling in water a hundred parts of the acetite of lead of the shops, with a hundred and fifty parts of litharge well dried, and deprived of carbonic acid by means of fire. Analysis corroborated the existence of two species of acetite of lead; one, long known, consists of oxide of lead 0.58, acetous acid 0.26, and water 0.16, the other, which has hitherto escaped the notice of chemists, contains oxide of lead 0.78, acetous acid, 0.17, water 0.05.

A manufacturer obliged to stop his work, because his acetite of lead was always in the forms of laminæ.

Two species of acetite of lead.

The former of these salts has an excess of acid, and a strongly saccharine taste; crystallizes in needly prisms, which appear to be hexagonal, and terminated by hexaedral pyramids; undergoes no alteration in the air; is very soluble in water, and forms with it a solution feebly precipitable by carbonic acid. The latter, on the contrary, is neutral; has a less perceptible saccharine taste; affects a lamellated figure; is soluble in vinegar, and then exhibits on evaporation the needly form;

The common or that with excess of acid.

The new or neutral acetite.

* Bulletin des Sciences, No. 77.

effloresces

effloresces slightly in the air; is much less soluble in water, but forms with it a solution abundantly precipitable by carbonic acid. This precipitate is very white, forms a paste with oil, and by extracting the carbonic acid from chalk by means of fire, it would perhaps be possible to prepare a fine white-lead by these means.

Fine white lead may be prepared from it.

Advantages of the discovery.

The value of the discovery of this salt, will be readily perceptible. It not only brings us acquainted with a new substance interesting in a scientific view, as it affords us a fresh proof, that the proportions of the constituent principles of salts may vary greatly; but it is likewise of importance in the art of physic, in which salts of lead are daily employed, and which may require one containing a large proportion of oxide; and it is of consequence to the arts in general, as it affords a new method of obtaining a fine white-lead, and particularly to that of manufacturing acetite of lead, on the processes and products of which it throws great light.

The Arachis Hypogæa, or Ground Nut of the West Indies, cultivated in France for its Oil.

Arachis hypogæa cultivated in France.

IN the departments of Landes and l'Herault in France, lat. 43½° to 44½°, an oily plant, called arachis, of the family of lentils, (arachis hypogæa) begins to be cultivated. It was brought by the Spaniards from Mexico, and was introduced by the French from Spain. An ounce of the oil of this plant, with a wick a line and half in diameter, burned nine hours and twenty-six minutes; an ounce of olive oil under similar circumstances, lasted only eight hours. Thus the oil of the arachis has the advantage of more than one eighth over olive oil; and it has more or less over every other kind of oil. It is an excellent substitute for olive oil for every domestic purpose, and is preferable to all other oils for the manufacture of soap. The seed yields nearly half its weight of oil.

Affords an oil excellent for lamps,

and other purposes.

*** The enquiries of *a correspondent*, respecting the method of experiment adopted by Dr. Irvine, will be answered in our next.

4

Perspective View of Mr. Woart's method of supporting Timbers.

Fig. 2.

Addition to the Sextant for aerostatic observations.

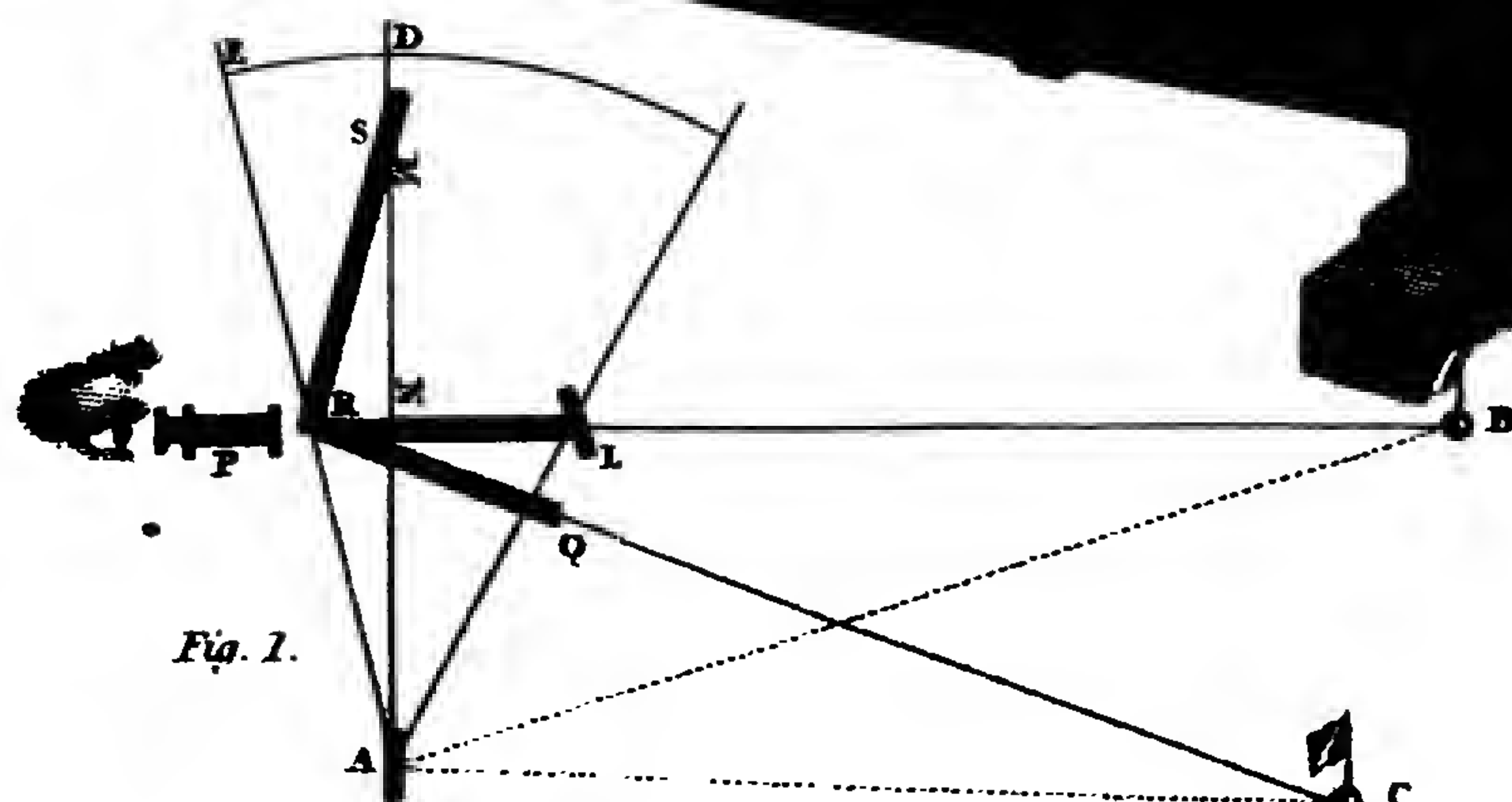
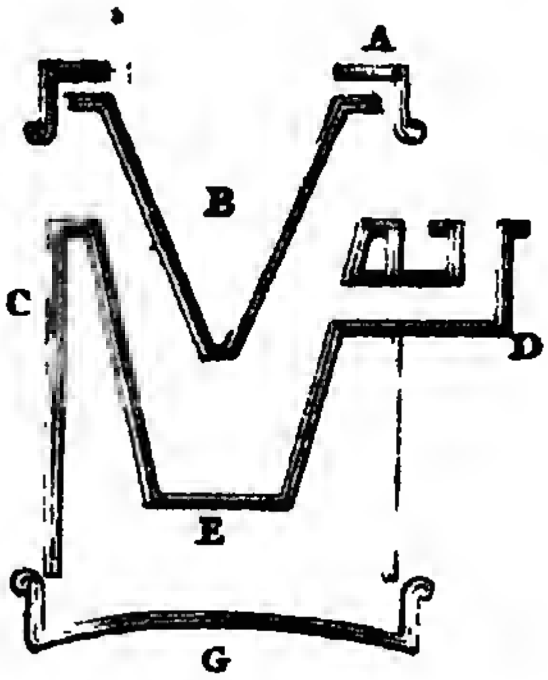


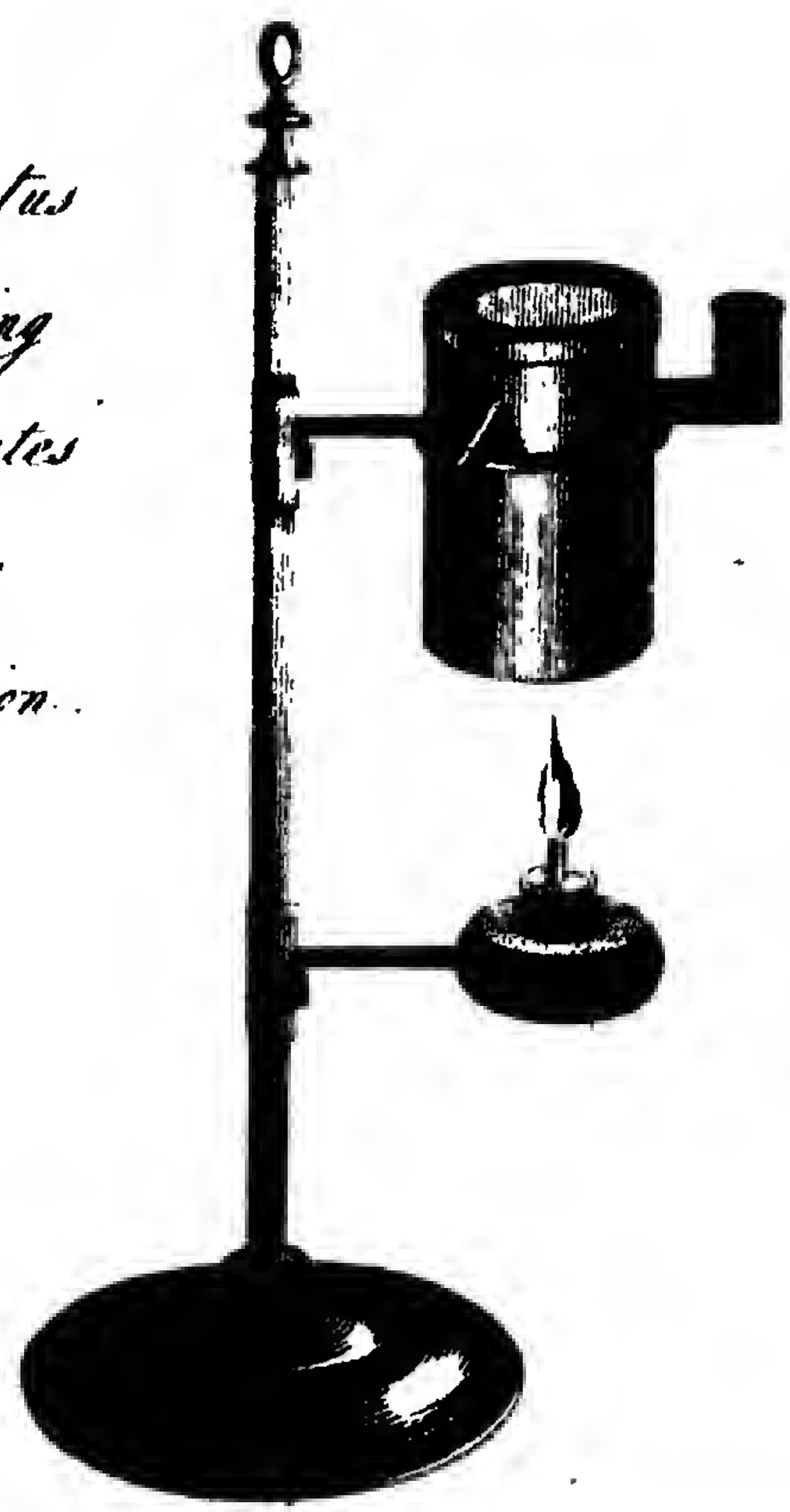
Fig. 1.

Drawn by Blunt

Engraved by Mutlow Russell sc.

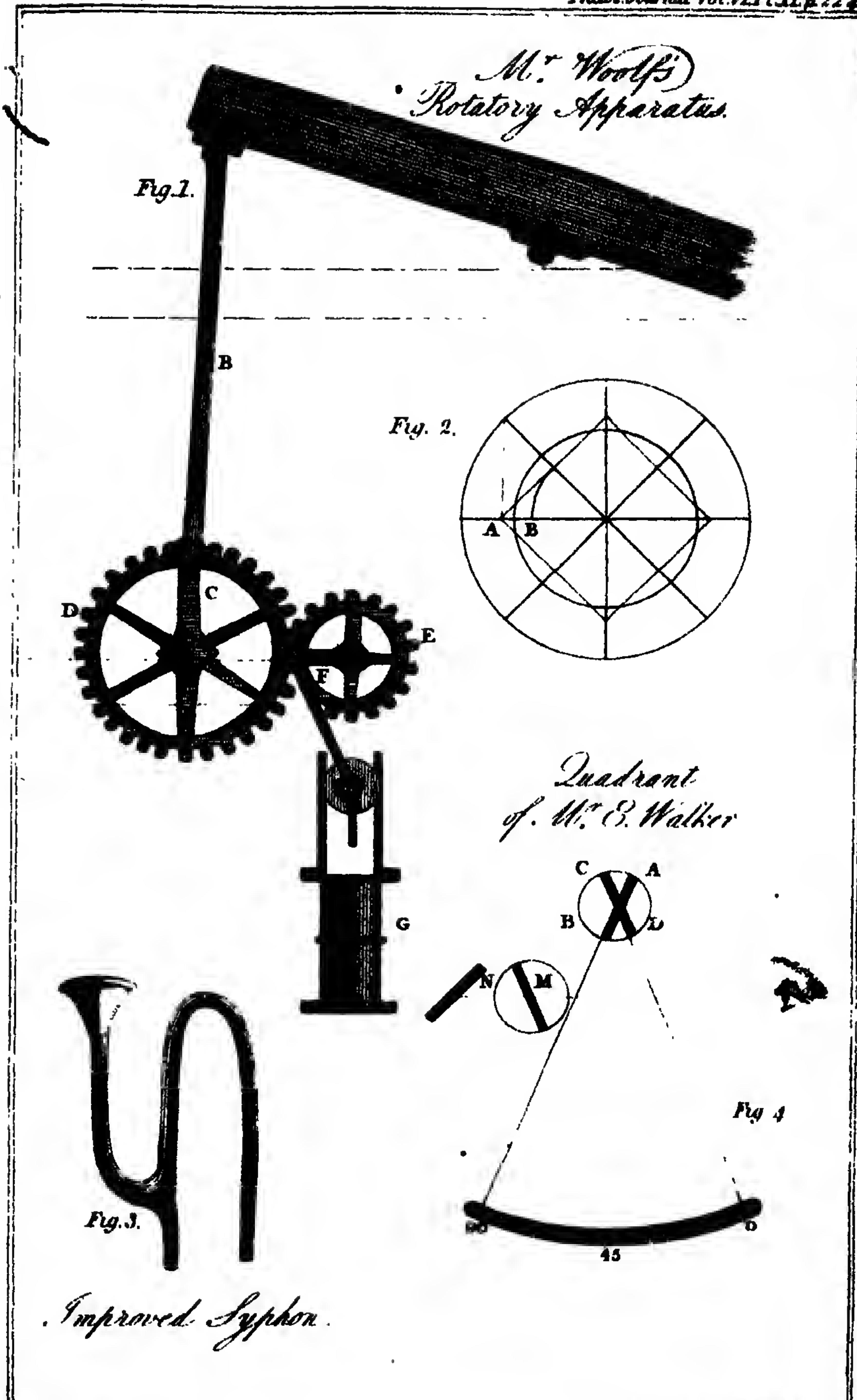


Apparatus
for drying
Precipitates
and for
Congelation.

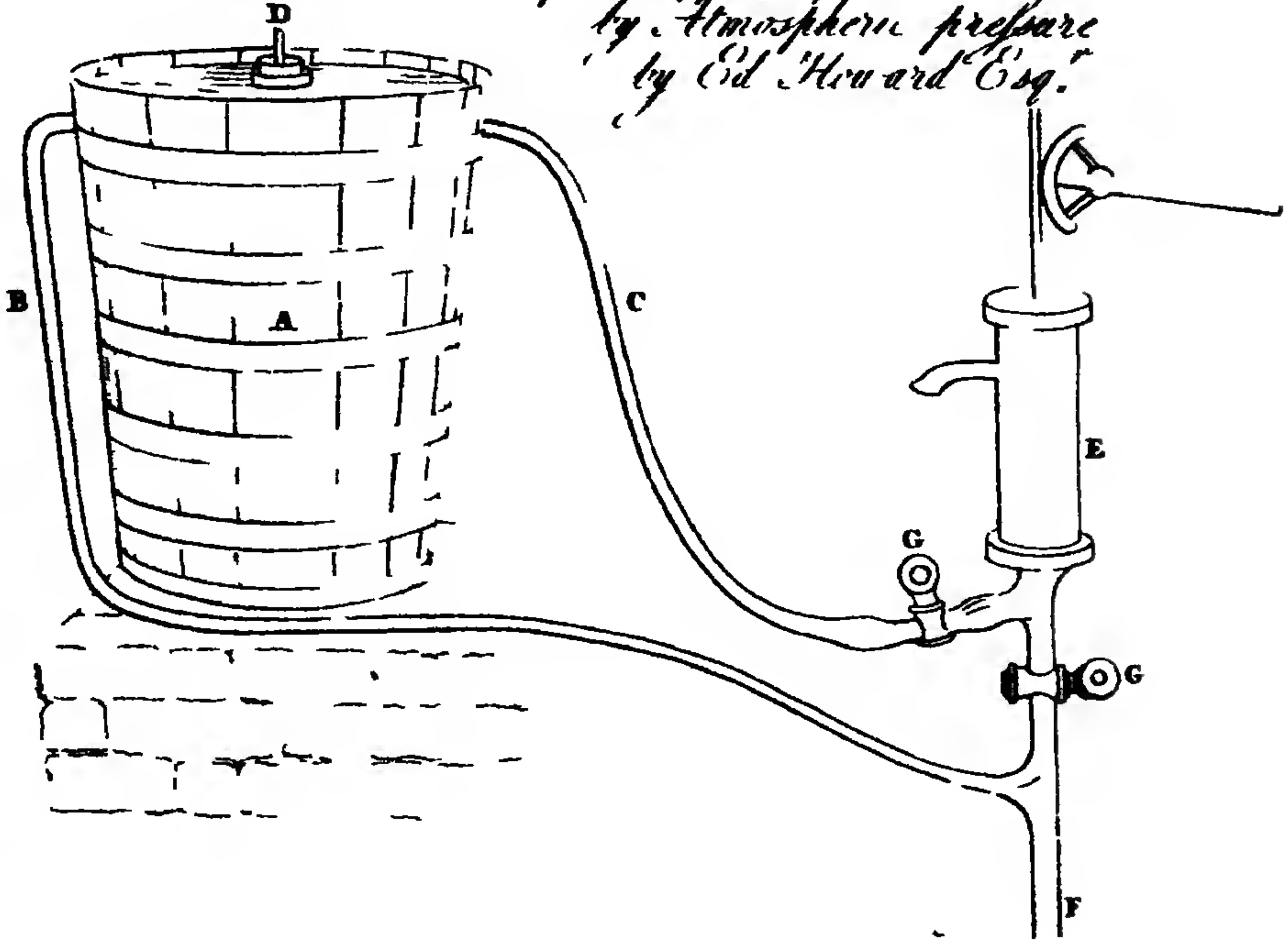


Drawn by Blunt.

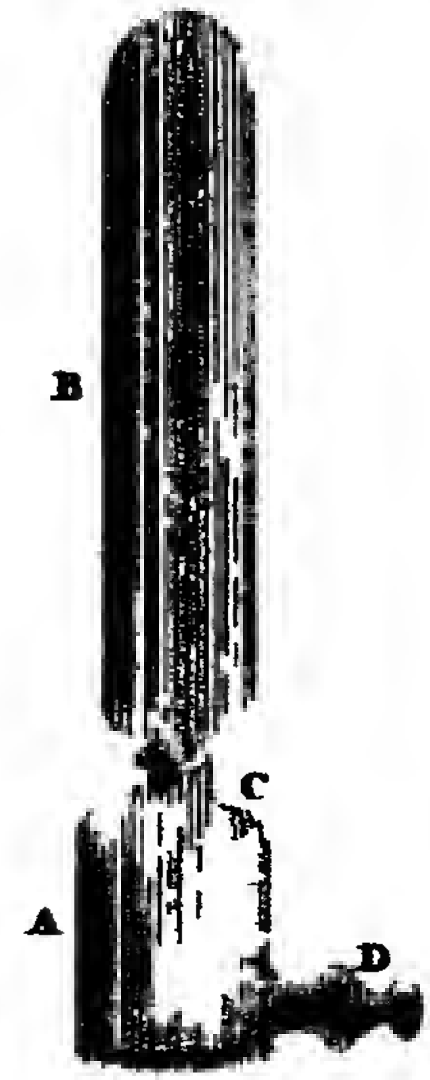
Engraved by Mutlow Russell Ct.


Mr. Woolf's Rotatory Apparatus.
Fig. 1.
B
C
D
E
F
G
Fig. 2.
A B
Quadrant of Mr. E. Walker
C A
B D
N M
Fig 4
45
Fig. 3.
Improved Syphon.

Experiment
for supplying Worm tubs &c
by Atmospheric pressure
by Ed Howard Esq.r



D.r Hopes
Eudiometric
Apparatus



Philos Journal Vol VI Pl XII p 22

Drawn by

Engraved by Hutton Russell

A
JOURNAL
OF
NATURAL PHILOSOPHY, CHEMISTRY,
AND
THE ARTS.

DECEMBER, 1803.

ARTICLE I.

Letter from ANDREW DUNCAN, *M. D. F. R. S. E. containing Experiments and Obſervations on Cinchona, tending particularly to ſhew that it does not contain Gelatine.*

To Mr. NICHOLSON.

SIR,

HAVING been long engaged in a ſeries of experiments on the aſtringent ſubſtances employed in medicine, I was particularly intereſted with the "Abſtract of a Memoir on the Febrifuge principle of Cinchona," contained in the laſt number of your excellent journal. The preſence of gelatine in cinchona, was ſo incompatible with experiments I had formerly made, that I was ſtrongly inclined to believe, that Seguin (than whom no one ſhould be better acquainted with the combinations of tannin and gelatine) had been miſled, either from having examined cinchona which had been adulterated, or from ſome other accidental cauſe. To ſatisfy myſelf I immediately proceeded to the unerring teſt of experiment, which has convinced me that cinchona does *not* contain gelatine, but ſome other principle not yet ſufficiently examined, which agrees with gelatine, in forming with tannin, a precipitate comparatively inſoluble in water. At the ſame time it is but fair to remark, that my experiments were made with the infuſion

Preſence of gelatine in cinchona, a miſtake.

But it contains ſome other principle, forming an inſoluble precipitate with tannin.

fusion and tincture of cinchona, containing all the soluble principles of that substance, whereas Seguin's observations are said to be derived from the examination of the isolated febrifuge principle, of which he gives the following characters: "It precipitates the solution of tan, but not the solutions of gelatine and sulphate of iron." On the contrary my experiments teach me, that the entire infusion and tincture of cinchona, precipitate the solution of tan, and also the solution of gelatine slightly, and the solution of sulphate of iron copiously. But as the two last precipitates may be reasonably ascribed to the action of other principles contained in my infusion and tincture of cinchona, I shall not insist upon them, but proceed to shew that, although cinchona actually does precipitate the solution of tan, yet it does not contain gelatine.

Infusion and tincture of cinchona, precipitate solutions of tan, gelatine, and sulphate of iron.

Experiment I.

Experiments in proof of this. Infusion of galls precipitated by infusion of cinchona.

(*a*) An ounce of infusion of galls was saturated, by adding to it in different portions, an ounce and a half of infusion of cinchona. The mixture was white and turbid, with a loose light precipitate.

(*b*) On filtration the fluid passed almost colourless, and perfectly transparent.

(*c*) The precipitate when dried, weighed five grains. It had a yellow colour, and an opaque earthy appearance, was extremely friable, and did not adhere to the filtering paper.

Gave a further precipitate with gelatine.

(*d*) The filtred fluid gave no further precipitate with solution of cinchona, but with half an ounce of solution of gelatine, containing six grains of gelatine in each ounce, it produced a copious precipitate, and was saturated.

(*e*) The precipitate, when separated by filtration, and dried also, weighed five grains, but was hard and brittle, adhered strongly to the paper, had a yellow colour, and exactly resembled a resin in appearance.

Experiment II.

Infusion of galls precipitated by gelatine.

(*a*) An ounce of the same infusion of galls was saturated by an ounce and a half of the same solution of gelatine. Immediately a very copious, whitish, tenacious, and adhesive precipitate was formed.

(*b*) On filtration the fluid passed very slowly, and even after repeated filtration, still retained a slight degree of opaline bluishness.

(*c*) The

(*c*) The precipitate when dried, weighed fourteen grains and a half. It had a brownifh yellow colour, was tranfparent, and had a refinous appearance and fracture. It was alfo hard and brittle, and adhered ftrongly to the filter. In every particular it refembled the precipitate produced in the former experiment (*Exp.* I. *c.*) by gelatine, after the infufion of galls was completely faturated by cinchona.

(*d*) In the filtred liquor (*Exp.* II. *b*) infufion of cinchona produced no change.

Infufion of cinchona gave no farther precipitate.

Experiment III.

To an ounce and a half of the fame infufion of cinchona, half an ounce of the folution of gelatine was added. It produced only a flight degree of turbidnefs, and changed the colour of the infufion from a pale greenifh to a reddifh yellow colour. When filtred, it paffed perfectly tranfparent, and the bottom of the filter was covered with a red varnifh; but it had gained only one grain in weight. In other experiments with larger quantities and ftronger infufion of cinchona, the prefence of tannin was more ftrongly indicated.

Infufion of cinchona with gelatine gave flight figns of tannin.

Experiment IV.

Infufion of galls was not affected by rectified fpirits of wine, in which ifinglafs had been long infufed.

Infufion of galls not affected by ifinglafs diffolved in alcohol.

Experiment V.

(*a*) A tincture of cinchona was prepared by infufing it in the fame rectified fpirits. After it was filtred fome refin was feparated by precipitation with water and filtration.

(*b*) With infufion of galls this tincture gave a copious precipitate, exactly refembling that produced by the fame reagent and infufion of cinchona. (*Exp.* I. *c.*)

Tincture of cinchona precipitated by water, and by infufion of galls,

Experiment VI.

With tincture of galls the fame tincture of cinchona gave no precipitate.

Tincture of galls and of cinchona gave no precipitate,

Experiment VII.

In the mixed tincture (*Exp.* VI.) a copious precipitate was produced by diluting it with water.

till diluted by water.

Experiment VIII.

Carbonate of potafh precipitates folution of gelatine,

A folution of carbonate of potafh produced a copious white flaky precipitate in the folution of gelatine, which was foluble in boiling water, but was not precipitated from the folution by infufion of galls, until fome acid was added.

Experiment IX.

but not infufion of cinchona.

The folution of carbonate of potafh changed the colour of the infufion of cinchona to a fine red, without difturbing its tranfparency.

Difference between gelatin and cinchonin.

Thefe facts feem to me fufficient to prove the difference between gelatine, and the new principle in cinchona, which for the fake of convenience, I fhall venture for the prefent to denominate cinchonin.

Gelatin forms a jelly with water,

Gelatine is foluble in water, and the folution is difpofed to gelatinize. Six grains of ifinglafs, diffolved in one ounce of water, form with it at temperatures below 60° Fahrenheit, a jelly of confiderable firmnefs. From its folution in water, gelatine is precipitated by alcohol, and a folution of carbonate of potafh. It is precipitated alfo by tannin, and the precipitates form a hard brown tranfparent mafs.

and is precipitated from it by alcohol, carbonate of potafh, and tannin.

Cinchonin does nor form jelly with water, is not precipitated by carbonate of potafh, is foluble in alcohol, and combines with tannin.

Cinchonin is foluble in water, but gives it no tendency to gelatinize. From its folution in water, it is not precipitated by a folution of carbonate of potafh. It is foluble in alcohol; it combines with tannin. The compound is foluble in alcohol, but forms, when water is added, or ufed as a menftruum, a friable opaque yellowifh precipitate; but cinchonin does not feparate even from a watery folution of tannin, all that is precipitable by a folution of gelatine.

ANDREW DUNCAN, Jun.

Edinburgh, 30th Oct. 1803.

II. *Letter*

II.

Letter from a Correſpondent, containing Diſquiſitions on the Phantaſms of NICOLAI, *and other Derangements of the Animal Syſtem.*

To Mr. NICHOLSON,

SIR,

THE account publiſhed in your laſt number by Mr. Nicolai, of his ſeeing ſpectres, appears not only to admit of an explanation on ſome of the laws of viſion with which we are already acquainted, but may alſo lead to ſome rational account of the grounds, on which the belief in apparitions, which has ſo generally prevailed in all ages of the world, may be founded.

The phantaſms of Nicolai may be explained,

If we look for a moment at the ſun, and afterwards turn our eyes on the blue ſky, or throw them upon the ground, we perceive a black ſpot of the apparent ſize of that luminary. This ſpot will by degrees aſſume a faint green colour, then becomes red; and if we pay attention to theſe phenomena, we ſhall find the green and red colours alternate, becoming gradually more faint, till they wholly diſappear.

—from the impreſſions of viſible objects on the eye.

If the eye be directed for ſome time towards a window, and then covered by the hand, the bars will firſt appear luminous, and the ſquares dark, and then the contrary, and this will alternate till the whole image gradually faints away.

Inſtance of a window.

Theſe are termed ocular ſpectra, and have been ſuppoſed to depend on the alternate tenſion and relaxation of the fibres of which the retina are compoſed. They reſemble the trembling of an over-fatigued muſcle, in which one ſet of fibres attempts to relieve the exertions of another. Theſe phenomena are moſt obvious after the eye has been fatigued by long continued exertion. They are more readily obſerved in the evening than the morning, and the ſpectra of the ſetting are more eaſily caught than of the meridian ſun.

Theſe ocular ſpectra are moſt obvious in the ſtate of debility or fatigue.

In certain ſtates of bodily debility, whether produced by the abſence of volition, or by diſeaſe, they are known ſpontaneouſly to occur. In the former ſtate, which may be termed reverie, when a perſon attends ſlightly to the impreſſions of his ſenſes, without attempting to regulate them by his will, as when a perſon looks careleſsly in the fire during twilight, the infinite

Spontaneous occurrence of ſpectra in the ſtate of reverie.

infinite variety of fantaſtic forms that paſs before the eyes is familiar to every one. Theſe fancies are however, in ſome meaſure, influenced by the train of thinking a perſon is accuſtomed to purſue. Swedenbourg appears to have been perpetually under the influence of this kind of reverie, except when employed in writing an account of what had previouſly occurred to his mind. Had he been conſtantly occupied in active bodily exertion, perhaps it would have diſſipated his phantaſies.

Swedenbourg perpetually in this ſtate.

Why dying perſons pick the bed-clothes, and imagine they ſee demons or dark figures.

Picking the bed cloaths, which appears to occupy the attention of thoſe who are labouring under the debility preceding death, probably ariſes from parts of the retina becoming inſenſible of the impreſſion of light, which produces the ſenſation of ſomething dark lying on the bed-cloaths, which they are deſirous to remove. The enfeebled mind may not unfrequently transform theſe into the figures of demons. The purſuit of the dark ſpot formed by the inſertion of the optic nerve, which in that ſtate is miſtaken for a reality, may alſo give occaſion to this action ſo frequently obſerved among the dying.

Nicolai evidently indulged the ſtate of reverie.

Mr. Nicolai informs us, that he was in the habit of forming vivid repreſentations to himſelf not only of whole ſcenes of comedies, but even of the peculiar dreſſes, forms, and complexions of thoſe who performed them; that is, he accuſtomed himſelf earneſtly to attend to theſe ideal exertions of his own imagination. In the next place he tells us, that he firſt began to ſee ſpectres after having laboured under a nervous fever, and great trouble of mind. For the melancholy caſt of his earlier viſions, or the appearance of dead bodies, his dejected ſtate of mind accounts. That appears however to have ſoon ſubſided. The diſeaſed ſtate of the retina, conſequent to his fever, ſeems to have continued longer.

The phantaſms were of things before ſeen.

This ſtate appears to have been ſuch as to render him ſenſible of the ſpectra of the things he was in the daily habit of ſeeing, ſuch as men, horſes, dogs, &c. for he does not ſay he ever ſaw any thing uncommon. What this particular condition of the retina might conſiſt in, it is impoſſible to determine. It may have been a combination of weakneſs with exceſs of ſenſibility. The ſpectra of the bodies he had ſeen involuntarily recurred to his attention, but not with ſuch ſtrength as to prevent the more forcible impreſſion of what was in reality paſſing before

before him. The notion of hearing the phantoms ſpeak I ſhould refer to an affection of the organ of hearing, ſimilar to what took place in the eye. It is evident they were both removed by a ſlight diminution of the quantity of circulating blood.

Audible deluſions.

Many ſtories of apparitions may, in my opinion, be accounted for on ſimilar principles. A perſon fixes his eyes intently on the face of an expiring friend illuminated by the light of a candle, perhaps with the intention of taking a laſt farewell; ſoon after, going into the dark, the ſpectrum of this luminous appearance occurs to the fatigued eye, and he thinks he perceives the dying man he had juſt left, ſtanding before him.

Phantaſms of departed friends.

On theſe principles we may venture to correct an error in the general repreſentation of our juſtly popular play of Hamlet. The ghoſt ſhould only appear once. This ſingle appearance makes ſo ſtrong an impreſſion on the mind of Hamlet, which, together with an habitual melancholy, was debilitated by care and vexation, that whenever afterwards he thinks ſeriouſly on his father, the ſpectrum of the ghoſt recurred to his eye, as he himſelf informs us, when he tells Horatio he ſees his father, and is aſked where, he ſays, "*in my mind's eye.*"

Correction of the repreſentation of Hamlet.

Theſe curſory remaks, Sir, are in ſome meaſure written to evince how much better it is to attempt at leaſt, to account for phenomena on principles already known, than to hunt for a new cauſe for every uncommon appearance. But what kind of philoſophiſing can we expect from a man, who quotes ſuch canons as the following, for rules of philoſophy? "That knowledge derived from experience is merely empirick, and therefore not to be depended on." "That obſervation ſhould not be admitted in theoretical philoſophy." What is theoretical philoſophy? After the exiſtence of Bacon, of Newton, and of Locke, who could have expected to live to ſee the whole thinking part of a nation puzzling themſelves about opinions, which, if they admit of any deſcription, may be characterized as a jumble of the abſtractions of Ariſtotle, with the ideal ſyſtem of Berkley?

Concluding remark.

A STUDENT.

III. Ex-

III.

Experiments on the Subſtance vulgarly called Gum Kino. By CIT. VAUQUELIN *.

Kino not a gum.

THE name given to this ſubſtance is by no means ſuited to it; and were it not a common practice, to give names to things before we are acquainted with their nature, it would be inconceivable how it ſhould have been called a gum, having neither the phyſical nor chemical properties of one.

Whence it is obtained not known.

Neither have we any accurate knowledge of the tree or of the country that produces it; but it appears to have been firſt brought to Europe by the Engliſh, who made known its medicinal properties, and introduced it into our ſhops.

Said to be from the pau de ſangue of Africa.

It is called in trade *kino* or the *gum-reſin* of Gambia. Dr. Oldfield, who made it known to Fothergill, termed it the true gum of Senegal. In the Medical Obſervations and Inquiries, it is ſaid to be brought from Africa, and the tree that furniſhes it to be called by the natives *pau de ſangue.*

Uſed in medicine as a tonic.

As a medicine it is uſed in the form of boluſſes, lozenges, aqueous infuſion, and ſpirituous tincture, as a tonic and aſtringent.

Yields an aqueous fluid, an oil, carbonic acid, and carbonated hidrogen.

Subjected to the action of fire, it melts and ſwells up conſiderably: yields at firſt a clear liquor, which in a few inſtants becomes coloured; a light and nearly white oil then paſſes over, which in the courſe of the proceſs becomes coloured and heavier than the aqueous product. A ſmall quantity of carbonic acid is likewiſe formed, with a large quantity of carbonated hidrogen gas.

Its oil.

The oil produced in this operation unites with cauſtic fixed alkalis, and forms a deep red liquor, that becomes of a dull green on expoſure to the air.

Its aqueous product.

The aqueous product is not acid, but has an acrid burning taſte, owing to a portion of the oil retained in ſolution; and potaſh ſeparates from it a large quantity of ammoniac.

Its reſiduum.

Twenty grammes diſtilled with a ſtrong heat left eight and half of a very bulky coal, marked with the colours of the rainbow; and this coal afforded ſeventy-two centigrammes of aſhes, conſiſting chiefly of lime, ſilex, alumin, and oxide of iron.

Abridged from the *Annales de Chimie.*

Kino

Kino is little ſoluble in cold water, but much more in hot, though a portion of it is inſoluble. The ſolution is ſlightly acid: alcohol does not precipitate it, but ſeparates ſome reddiſh flocks; when made with boiling water it grows turbid on cooling, and depoſits a brown red precipitate.

Its ſolution in water acidulous.

A ſaturated ſolution is precipitated by mild alkalis, but water in ſufficient quantity re-diſſolves the precipitate.

Precipitated by mild and cauſtic alkalies,

Cauſtic alkalis likewiſe precipitate it, but if added in exceſs re-diſſolve the precipitate.

Glue diſſolved in water forms a very conſiderable roſe-coloured coagulum with the ſolution of kino; and if the quantities be ſuch, that both ſubſtances are ſaturated, the ſupernatant fluid will be nearly colourleſs.

and coagulated by glue.

Though theſe appearances indicate the preſence of tannin in kino, it does not precipitate ferruginous ſalts black, but of a beautiful deep green, ſcarcely alterable by expoſure to the air. This property it has in common with the infuſions of cinchona and rhubarb; whence it is probable, that theſe three ſubſtances contain a principle of ſimilar nature.

Its tannin precipitates iron green.

Contains a principle common to i, bark, and rhubarb.

This principle, whatever it be, is very deſtructible: for, if a little oxigenated muriatic acid be poured on the precipitate it forms with iron, this loſes its colour, and does not re-appear on the addition of an alkaline carbonate, which produces only a red oxide of iron.

This principle very deſtructible.

The ſolution of kino copiouſly precipitates acetite of lead of a yellowiſh grey; nitrate of ſilver of a reddiſh yellow, and tartrite of antimony of a yellowiſh white, but much more copiouſly than the infuſion of tan or of cinchona; which ſeems to indicate, that it would be a better antidote in caſes of perſons poiſoned by this metallic ſalt.

Precipitates ſome metallic ſolutions.

Uſeful as an antidote.

Wool and cotton being boiled in a ſolution of kino, and then dipped in a bath of ſulphate of iron, appeared on immerſion of a bottle-green; but being waſhed and dried, the colour became a blackiſh brown. It was very durable.

Dyes wool and cotton a blackiſh brown.

Hot alcohol diſſolves kino very well, all but a ſmall portion. Water renders the ſolution a little turbid, but precipitates nothing.

Diſſolves in hot alcohol.

The portion inſoluble in alcohol, nearly a fourth of the whole, has neither the bitterneſs nor aſtringent taſte of kino; but, on the contrary, is rather mucous and ſweet. It eaſily diſſolves

Contains a portion of gum.

in hot water, and gives it a fine red colour. It is precipitable by alcohol; but neither by glue, nor by any metallic ſolution. On burning, it diffuſes a ſmell reſembling that of gum.

This favours the ſolution of the other principle in water.

I ſuſpect the preſence of this ſubſtance favours the ſolution in water of the principle ſoluble in alcohol; for the latter is leſs ſoluble in water when ſeparated from the former; and, if the quantity of water neceſſary for diſſolving the aſtringent part be not employed in the firſt inſtance, what is left requires a greater proportion of water.

Water diſſolves ⅘, alcohol moſt of the remainder.

Four litres of water, uſed at different times, left near twenty grammes out of a hundred of kino undiſſolved. The reſiduum grew ſoft like a reſin in boiling water, and all of it, except ſeven decigrammes, was ſoluble by alcohol, to which it imparted all the properties before obſerved in the aſtringent matter.

Sulphuric acid renders it leſs ſoluble.

Sulphuric acid diminiſhes the action of water on kino, inſtead of increaſing it, as it does with reſpect to the reſinous part of cinchona.

It tans leather.

It is capable of being uſed for tanning leather.

Neither a gum, nor a gum-reſin, but chiefly a ſpecies of tannin.

From what has been ſaid it appears, that the greater part of kino conſiſts of tannin, and is neither a gum, nor a gum-reſin. But there is a ſlight difference between it and the tannin of galls and oak bark, which precipitate iron of a blue black, while kino precipitates it green, in which it reſembles cinchona and rhubarb. If therefore it were to become plentiful and cheap, it might be employed for all the purpoſes for which aſtringent vegetables are commonly uſed.

Addition.

Dr. Duncan firſt aſſerted it to be an extract.

Mr. Vauquelin is not the firſt who diſcovered the common error reſpecting kino. In the new Edinburgh Diſpenſatory, Dr. Duncan has entered pretty fully into the ſubject, and aſſerts it to be in reality an extract. He adds, that what we have now in the ſhops is not brought from Africa, but chiefly from Jamaica. In a private letter he informs me, that this is an extract of the *coccoloba urifera*, or ſea-ſide grape; while the fineſt kino of the ſhops, and what from ſome circumſtances he ſuppoſes was the ſort analyzed by Vauquelin, is the product of different ſpecies of *eucalyptus*, particularly the *reſinifera*, or brown gum tree of Botany Bay, from which country a parcel was imported ſome years ago.

Obtained from the ſea-ſide grape, and gum-tree of Botany Bay.

W. N.

IV. *Extract*

IV.

Extract of a Letter from DR. PRINCE, *respecting his Air-Pump.*

IT is unnecessary here to enter into the merits of Dr. Prince's air-pump, as they were noticed in the 1st. Vol. of the Philosophical Journal, 4to. edit. p. 130. The purpose of his letter is to defend himself against the misrepresentations of the writer of the article pneumatics, in the Encyclopedia Britannica; which he has done in the supplement to the edition of that work reprinting in America, where it may be presumed it will meet the eye of but few English readers.

Dr. Prince's air-pump misrepresented in the Encyclopedia Britannica.

The Encyclopedia says, "great inconveniences were experienced from the oscillations of the mercury in the gauge. As soon as the piston comes into the cistern, the air from the receiver immediately rushes into the barrel, and the mercury shoots up in the gauge, and gets into a state of oscillation. The subsequent rise of the piston will frequently keep time with the second oscillation, and increase it. The descent of the piston produces a downward oscillation, by allowing the air below it to collapse; and by improperly timing the strokes, this oscillation becomes so great, as to make the mercury enter the pump."

Charged with inconvenience from oscillations of the mercury in the gauge.

This, Dr. Prince observes, is a very singular account of the working of the American air-pump. It *seems* to be founded on experiment, yet it is contradicted by numerous experiments performed with the original pump, and with one on the same construction, made by the late Mr. G. Adams in London. Many scientific and respectable persons were witnesses, that no such extraoscillations were produced by it; but that the mercury rose in the gauge in the same manner as it did in a double-barrelled pump of the common construction made by Nairne. Add to this, Mr. Adams, who made the first pump in England on this plan, mentions no such effect, either in his letter to the inventor, or in his public account of it; nor does Mr. Jones, who has since made pumps on this plan, and given an account of their exhausting power, which he says, in a letter to Dr. Prince, is fully equal to that of Cuthbertson's.

This contradicted by facts.

To

Valves said to be put in the piston to prevent downward oscillation;

To prevent the downward oscillation, which, Dr. Prince observes, could only occur in a single-barrelled pump, and which he obviated by using two barrels, the Encyclopedia says, " valves were put into the piston ; but as these require force to open them, the addition seemed rather to increase the evil, by rendering the oscillation more simultaneous with the ordinary rate of working." If, replies the Doctor, such an evil were produced by the descent of the piston, it is difficult to conceive, how putting valves into the piston could have increased it. They could not increase the evil, unless they increased the resistance to the air under the piston. But it must be a strange assertion, that a piston with a *valve* in it will give more resistance to the air than a *solid* piston.

but valves could not increase its resistance.

Said to be difficult of execution;

Another objection is : " it appears of very difficult execution. It has many long, slender, and crooked passages, which must be drilled through broad plates of brass, some of them appearing scarcely practicable. It is rare to find plates and other pieces of brass without air-holes, which it would be difficult to find out and close, &c. Now the fact is, there is not so much pipe and duct-work in the American air-pump, by more than one half, as in Mr. Nairne's improved pump of Smeaton, against which no such objection is brought. There can be no reason to presume the brass work would be more defective, unless it were more complex in its form. And it is obviously far superior in point of simplicity to Cuthbertson's, which the Encyclopedia considers as " the most perfect that has yet appeared."

but less so than others.

After these strictures on the Encyclopedia Britannica, Dr. Prince gives an account of the improvements he has made on his former attempt.

Subsequent improvements.

' The following improvements have been made in the American air-pump, by the inventor, to render it more simple and convenient. It has been observed above, that in all air-pumps, made to condense as well as exhaust by the same barrels and winch, there must be additional pipes, ducts and cocks to command and regulate the operations : But this is not the best method of constructing the instrument for exhausting and condensing experiments : for a great strain is brought upon the rackwork of the pump when several atmospheres are throw into the receiver : and the pump may be made with less trouble and expense by fixing a common con-

densing

denſing ſyringe to it, in the following manner. Let a ſtraight pipe be fixed to the ciſterns, and paſs horizontally to the receiver-plate, as in the common table air-pump. At a convenient diſtance from the barrels this pipe muſt be ſwelled out ſo as to admit the key of a ſtop-cock. The key of this cock muſt be pierced quite through in the direction of its handle; and half way through, at a right angle to meet the other hole. A ſmall pin muſt be fixed in the handle, on that ſide which correſponds with the ſhort hole. A hole muſt be made in the ſide of the pipe to correſpond occaſionally with the holes in the key. This cock is more ſimple than the one in the original pump, and will regulate the exhauſting and condenſing experiments. To ſet the cock for exhauſting the receiver, bring the handle of the key parallel with the pipe, with the ſolid part of the key againſt the hole in the ſide of the pipe; then will the communication be opened between the barrels and receiver, and the receiver may be exhauſted. To reſtore the equilibrium, or let the air into the receiver, ſet the handle of the key at right angles with the pipe, and let its projecting pin point to the receiver; then will the communication be opened between the atmoſphere and receiver, through the hole in the ſide of the pipe and the cock. In this ſituation the ſolid part of the key will cloſe the paſſage in the pipe leading to the barrels. If a condenſer, having a valve at its end, be now attached to the ſide of the pipe, oppoſite the hole, the air may be forced into the receiver through the cock without entering the barrels. The ſwelled part of the pipe, in which the key is inſerted, ſhould be ſo made as that the condenſer may be ſcrewed on or off, at pleaſure. The equilibrium may be reſtored in the receiver, either by unſcrewing the condenſer a little, or by letting the air out through the barrels.

Beſt made to condenſe by means of a ſyringe.

Stop-cock for regulating the experiments.

Mode of condenſing.

'In this conſtruction, the pipe ſtanding between the barrels in the original pump, and the drilled paſſages in the horizontal piece connecting this pipe with the regulating cock, are unneceſſary. The pump is rendered more ſimple, and every difficulty of execution on account of crooked paſſages, &c. removed. This alteration in the American air-pump was contrived by its inventor, and a table-pump made on this plan, for him, by the late Mr. G. Adams, before the laſt edition of the Encyclopedia was printed.

The pump thus rendered more ſimple.

'Another

Alteration in the valve-pump.

'Another alteration, ſince made, is in the ſituation of the valve-pump: the laſt mentioned pump not having one fixed to it. In all air-pumps having the tops of the barrels cloſed with plates and collars of leather, as in Nairne's, Cuthbertſon's, and the American pump (as now altered by removing the middle pipe,) it is neceſſary to connect oil boxes with the top-plates to receive the oil which is thrown out of the barrels in working the pump. Cuthbertſon's pump has two, one to each barrel. By removing the pipe from between the barrels, in the Americen pump, a ſmall barrel is ſcrewed in its place to the croſs-piece, which connects the top-plates covering the valves. The barrel anſwers the purpoſe of an oil-box in common exhauſtions. When greater vacuums are wanted in the receiver, this barrel anſwers alſo for a valve-pump. On the top of the croſs-piece is ſcrewed a collar of leathers containing a piſton and its rod, to work occaſionally in the barrel below. At the lower end of the barrel is a valve covered with a cap: by unſcrewing the cap, and paſſing down the piſton, all the oil in the barrel is expelled through the valve; and afterwards the barrel, and the ſpace above the valves on the top-plates of the great barrels, are exhauſted of air, by working this ſmall pump. The ſmall piſton when drawn up to its collar of leathers is above the holes in the croſs-piece leading from the valves. When the ſmall barrel is uſed only as an oil-box, the collar of leathers, with the piſton, is removed, and a button, with a ſhort pipe in it, ſcrewed in its place to give vent to the air when expelled from the barrels: In this valve-pump there is not ſo much work as in Cuthbertſon's two oil-boxes; nor is it an additional expence; for the ſyringe, which is uſed with the lead weight in the receiver, is made to ſcrew to the croſs-piece for this purpoſe; the weight being taken off, and a cap ſcrewed on over the valve, when uſed as an oil-box. In the collars of leathers, on the tops of the barrels, are put two ſmall flat boxes, below one or two rings of the leathers, the piſton rods paſſing through them. Theſe boxes contain the oil to keep the leathers moiſt, and air-tight. In this ſituation the oil is not thickened by evaporation, nor carried up from off the leathers, when the piſton riſes, as in Nairne's pump, and the leathers are better ſupplied than by the dirty oil paſſing through the pump and returned to the collars by Cuthbertſon's crooked pipes. The American air-pump, made in this manner, is the ſimpleſt form of any pump of equal power.'

Barrel to anſwer the purpoſe of an oil-box, and alſo a valve-pump.

This no additional expence.

The oil neither thickened by evaporation, nor carried off from the leathers.

Memoir

V.

Memoir on the Tides. By Cit. Laplace *.

THE object of this paper is to compare the high tides obſerved on the 23d of March laſt, with the reſults indicated by the theory of univerſal gravitation.

New moon, in perigeum, and equinoctial ſyzygy, moſt favourable to high tides.

At this period the moon was new, and in her perigeum. Theſe circumſtances, joined to thoſe of an equinoctial ſyzygy, are the moſt favourable to high tides: and if at ſuch a time the action of the winds ſhould combine with theſe regular cauſes, inundations may follow, againſt which it is prudent to uſe precautions. It is with this view that the board of longitude publiſhes in the *Connoiſſance des Tems* every year, a table of the higheſt tides that follow every new and full moon.

Height of tide the difference between ebb and flood.

Wind acts equally on both.

To know the real height of the tide produced by the action of the ſun and moon, and diſtinguiſh it from that which is owing to the temporary action of the wind, it is not ſufficient to obſerve the abſolute height at flood, but the correſpondent ebb muſt be obſerved likewiſe, and the difference between them gives the total height of the tide. We can eaſily conceive, that the action of the wind muſt add to the height of the water, both at ebb and flood, nearly in an equal degree. This conſideration is indiſpenſable, for without it all we can deduce from obſervation is the ſum of the combined effects, without being able to ſeparate and refer them to their real cauſes.

Tide of 23d of March.

Sept. 23, 1714.

The tides of the 23d of March were obſerved at Breſt by Cit. Rochon and Mingon. The total height was 23 feet four inches, the greateſt ever obſerved. That which came neareſt to it, was as far back as the 23d of September 1714, when the moon was full, in her perigeum, and almoſt without declination, as well as the ſun. Its total height was 22 feet 11 inches.

Theory in *La Mecanique Celeſte* agreeable to obſervation.

According to the theory given in the fourth book of *La Mécanique Céleſte*, the greateſt difference between high and low water in the preceding ſyzygies, is 22 feet 10 inches, which differs very little from the obſervations: But in that book it is remarked, that the local circumſtances of every harbour may occaſion the relation of the action of the ſun and moon on the

* National Inſtitute of France, IV.

phenomena of the tides to vary. A comparifon of the obfervations made at Breft, has made known to Cit. Laplace, that circumftances there increafe the action of the moon one-fixth; and with this modification the refult of the theory is a mean between thofe given by obfervation.

Time of high-water at Breft has not varied in near a century.

The high tide of the 15th of September 1715, in the morning, and that of the 23d of March laft, in the evening, were nearly equidiftant from the fyzygy, which fhould give the fame hour for the tides, if the local circumftances of the harbour have not varied in the interval of nearly a century. The firft was obferved at half after four in the morning, true time; the fecond, at 29 minutes after four in the evening: whence it appears, that the time of the tides at Breft has not varied in that period.

Series of obfervations on the tides propofed.

Cit. Laplace has propofed to the firft clafs of the Inftitute, to folicit government to direct a feries of obfervations to be made on the tides in the different harbours of France; and to appoint a committee to draw up a fingle body of inftructions for the beft mode of making thefe obfervations. Both propofals were adopted.

The whole of the paper, of which an abftract is here given, will be printed in the *Connoiffance des Tems.*

VI.

Abftract of a Paper by Cit. GUYTON-MORVEAU, *entitled an Examination of a native Carbonate of Magnefia* *.

Magnefia with carbonate generally in fmall quantity in ftones.

THOUGH magnefia is a conftituent part of many ftones, it enters into them but in fmall quantity, with few exceptions. Native carbonate of magnefia occurs ftill more rarely in any confiderable proportion. Citizen Guyton, however, fearching for a clay poffeffed of the hygrometric property in the higheft degree, lately found a very large proportion of native carbonate of magnefia in a ftone in the vicinity of Caftella-Monte, which is there confidered as a clay very rich in alumine.

Characters of the ftone of Caftella Monte.

This ftone is as compact as the hardeft chalk, in an amorphous mafs, and as white as cerufe. It does not fenfibly adhere to the tongue, and has no argillaceous fmell. Water acts

* Bulletin des Sciences, No. 75.

very

very flightly on it; it is not reducible to a folid pafte; yet on drying it appears to agglutinate, and contract a little its dimenfions. Its fpecific gravity, when all the bubbles of air it contains have efcaped, is 2.612. In the fire it loft 0.585 of its weight, and became fufficiently hard to fcratch Bohemian glafs flightly. Five grammes, being diffolved in nitric acid, gave out a large quantity of gas, by which the weight was diminifhed 230 centigrammes.

Concentrated fulphuric acid, poured on the ftone reduced to powder, occafioned a violent effervefcence on the addition of water. Without this addition the effect was not perceptible. Effervefces with fulphuric acid,

By this treatment a falt cryftallized in fmall needles was obtained, which difplayed all the properties of fulphate of magnefia. and yields fulphate of magnefia.

This falt was precipitated by potafh, and the precipitate, when dried, weighed 131.5 centigrammes.

The portion not diffolved by fulphuric acid was pure filex, and weighed 71.2 centigrammes. Contains filex.

Pruffiat of foda gave the folutions a flight greenifh tinge, but nothing capable of being weighed was depofited.

This ftone therefore contains, Its analyfis.

Magnefia	26.3
Silex	14.2
Carbonic acid	46
Water	12
Iron	0
Lofs	1.5
	100.0

Cit. Guyton obferves, that the difference between the proportions of the conftituent fubftances of this ftone, and thofe of the artificial carbonate of magnefia, arifes no doubt from the circumftances in which thefe falts have been produced; and the other characteriftics, that diftinguifh them, may be occafioned by the prefence of the other fubftances found with the carbonate of magnefia in the ftone of Caftella-Monte. How it differs from the artificial carbonate.

VII.

Curious Particulars respecting the Mountains and Volcanos, and the Effect of the late Earthquakes in South America, with Remarks on the Language and Science of the Natives, and other Subjects. By M. A. VON HOMBOLDT *.

Three branches of the Andes.

WE arrived at Quito, by crossing the snows of Quridien and Tolima, for as the cordillera of the Andes forms three separate branches, and at Santa Fe de Bogoto, we were on the easternmost, it was necessary for us to pass the loftiest, in order to reach the coast of the Pacific ocean. We travelled on foot, and spent seventeen days in these deserts, in which are to be found no traces of their ever having been inhabited. We slept in huts made of the leaves of the heliconia, which we carried with us for the purpose. Descending the Andes to the west, there are marshes, in which you sink up to the knees. The latter part of the time we were deluged with rain; our boots rotted on our legs; and we arrived barefoot at Carthago, but enriched with a fine collection of new plants, of which I have a great number of drawings.

Huts made of the leaves of heliconia.

The Andes marshy toward the west.

Mines of Platina in mount Choca.

From Carthago we went to Popayan, by way of Buga, crossing the beautiful vale of the river Cauca, and having constantly at one side the mountain of Choca, in which are the mines of Platina.

Basaltic mountains.

Volcano of Purace.

Columnar porphyritic granites.

We staid during the month of November 1801, at Popayan, visiting the Basaltic mountains of Julusuito; the mouths of the volcano of Purace, which evolve, with a dreadful noise, vapours of hydrosulphurated water; and the porphyritic granites of Pische, which form columns of five, six, or seven sides, similar to those I remember I saw in the Euganean mountains in Italy, which Strange has described.

Paramos piercing cold and destitute of vegetation.

In travelling from Popayan to Quito, we had to cross the *paramos* of Pasto, and this in the rainy season. Every place in the Andes, where, at the height of 3500 or 4000 yards, vegetation ceases, and the cold penetrates to the very marrow of your bones, is called a *paramo*. To avoid the heats

* Abridged from the Magazin Encyclopédique.

of

of the valley of Patia; where, in a ſingle night, a fever may be caught, that will laſt three or four months, we paſſed the ſummit of the Cordillera, traverſing frightful precipices.

We ſpent our Chriſtmas at Paſto, a little town at the foot of a tremendous volcano, where we were entertained with great hoſpitality. The roads leading to and from it are the moſt ſhocking in the world. Thick foreſts, between marſhes, in which the mules ſink up to their bellies; and gullies ſo deep and narrow, that we ſeemed entering the galleries of a mine. Town of Paſto.

The whole province of Paſto, including the environs of Guachucal and Tuqueres, is a frozen plain, nearly beyond the point where vegetation can ſubſiſt, and ſurrounded by volcanos and ſulphur-pits, continually emitting volumes of ſmoke. The wretched inhabitants of theſe deſerts have no food but potatoes: and if theſe fail, as they did laſt year, they go to the mountains to eat the ſtem of a little tree, called *achupalla (pourretia pitcarnia)*; but the bears of the Andes, as they too feed on it, often diſpute it with them. On the north of the volcano of Paſto, I diſcovered, in the little Indian village of Voiſaco, 1900 yards above the level of the ſea, a red porphyry, with baſe of argil, encloſing vitreous feldſpar, and hornblende; that has all the properties of the ſerpentine of the *Fichtelgebirge*. This porphyry has very diſtinctly marked poles, but no attractive power. Near the town of Ibarra, we nearly eſcaped being drowned by a very ſudden ſwell of the water, accompanied with ſhocks of an earthquake. The province a frozen plain. The people live on potatoes, and the ſtems of the pouretia pitcarnia. Red porphyry with diſtinct poles.

We reached Quito on the 6th of January 1802. It is a handſome city; but the ſky is commonly clouded and gloomy. The neighbouring mountains exhibit little verdure, and the cold is very conſiderable. The great earthquake on the 4th of February 1797, which changed the face of the whole province, and in one inſtant deſtroyed thirty-five or forty thouſand perſons, has ſo altered the temperature of the air, that the thermometer is now commonly 41° to 54°, and ſeldom riſes to 68° or 70°, whereas Bouguer obſerved it conſtantly at 66° or 68°. Since this cataſtrophe, earthquakes are continually recurring; and ſuch ſhocks! it is probable, that all the higher ground is one vaſt volcano. What are called the mountains of Cotopoxi and Pichincha, are but little Quito. Earthquake of 1797. Altered the climate greatly. The heights one vaſt volcano.

fummits, the craters of which, form different conduits terminating in the fame cavity., The earthquake of 1797, afforded a melancholy proof of this; for the ground then opened every where, and vomited forth fulphur, water, &c. Notwithftanding the dangers and horrors that furround them, the people of Quito are gay, lively, and fociable, and in no place did I ever fee a more decided and general tafte for pleafure, luxury, and amufement. Thus man accuftoms himfelf to fleep tranquilly on the brink of a precipice.

People of Quito.

Pichincha.

I was twice at the mouth of the crater of Pichincha, the mountain that overlooks the city of Quito. I know of no one but Condamine, that ever reached it before; and he was without inftruments, and could not ftay above a quarter of an hour, on account of the extreme cold. I was more fuccefsful. From the edge of the crater rife three peaks, which are free from fnow, as it is continually melted by the afcending vapour. At the fummit of one of thefe I found a rock, that projected over the precipice, and hence I made my obfervations. This rock was about twelve feet long, by fix broad, and ftrongly agitated by the frequent fhocks, of which we counted eighteen in lefs than half an hour. We lay on our bellies, the better to examine the bottom of the crater. The mouth of the volcano forms a circular hole, near a league in circumference, the perpendicular edges of which are covered with fnow on the top. The infide is of a deep black; but the abyfs is fo vaft, that the fummits of feveral mountains may be diftinguifhed in it. Their tops feemed to be fix hundred yards below us, judge then where their bafes muft be. I have no doubt but the bottom of the crater is on a level with the city of Quito. Condamine found it extinct, and even covered with fnow; but we had to report the unpleafant news, that it was burning. On my fecond vifit, being better furnifhed with inftruments, I found the diameter of the crater to be 1600 yards, whereas that of Vefuvius is but 670. The height of the mountain is 5280 yards.

Its crater.

Several mountains within it.

Its diameter 1600 yards; height 5280.

When we vifited the volcano of Antifana, the weather was fo favourable, that we reached the height of 5915 yards. In this lofty region, the barometer funk to 14 inches 7 lines, [15.6 Eng.] and the tenuity of the air occafioned the blood to iffue from our lips, gums, and even eyes: we felt extremely feeble, and one of our company fainted away. The

Volcano of Antifana, 5915 yards.

Barometer at 15.6 inches.

Hemorrhages brought on.

4 air

air brought from the loftieſt point we viſited, gave on being analyſed 0.218 of oxigen gas, and 0.008 of carbonic acid. Air 0.218 of oxigen.

We viſited Cotopoxi, but could not reach the mouth of the crater. The aſſertion, that this mountain was diminiſhed in height by the earthquake of 1797, is a miſtake. Cotopoxi not ſunk by the earthquake of 1797.

In June we proceeded to meaſure Chimboraco and Tunguragua, and take a plan of all the country affected by the grand cataſtrophe of 1797. We approached within about 500 yards of the ſummit of Chimboraco, our aſcent being facilitated by a line of volcanic rocks bare of ſnow. The height we reached was 6465 yards; and we were prevented from aſcending farther by a chaſm too deep to croſs. We felt the ſame inconveniences as on Antiſana; and were unwell for two or three days after. The air at this height contained 0.20 of oxigen. The trigonometrical meaſurement I took of the mountain at two different times, and I can place ſome confidence in my operations, gave me for its height 6970 yards, a hundred more than Condamine aſſigns it. The whole of this huge maſs, as of all the high mountains of the Andes, is not granite, but porphyry, from the foot to the ſummit, and there the porphyry is 4050 yards thick. Air at 6465 yards, contained 0.20 of oxigen. Chimboraco 6970 yards high. Conſiſts of porphyry.

Chimboraco is probably a volcanic mountain, for the track by which we aſcended, conſiſts of a burnt and ſcorified rock mixed with pumice-ſtone, reſembling all the ſtreams of lava in this country, and ran higher up the mountain than we could climb. The ſummit therefore is in all likelihood the crater of an extinct volcano. Chimboraco a volcano.

The mountain of Tunguragua has diminiſhed in height ſince the earthquake of 1797. Bouguer aſſigns it 5589 yards, I found it but 5399, ſo that it muſt have loſt 190 yards; and indeed the people in the vicinity ſay, that they have ſeen its ſummit crumble away before their eyes. Tunguragua diminiſhed in height. Now 5399 yards.

During our ſtay at Riobancha, we accidentally made a very curious diſcovery. The ſtate of the province of Quito, previous to its conqueſt by the Inca Tupaynpangi, in 1470, is wholly unknown: but the king of the Indians, Leandro Zapla, who reſides at Lican, and has a mind extraordinarily cultivated for an Indian, poſſeſſes manuſcripts compoſed by one of his anceſtors, in the ſixteenth century, which contains the hiſtory of that period. They are written in the Parugay tongue, Indian manuſcripts of the 16th century.

tongue, which was formerly general in Quito, but is now lost, having been supplanted by the Inca or Anichna. Fortunately another of Zapla's ancestors amused himself by translating these memoirs into Spanish. We have obtained from them valuable information, particularly in the memorable period of the eruption of Nevado del Atlas, which must have been the highest mountain in the world, loftier than Chimboraco, and called by the Indians *Capa-urcu*, or chief of mountains. These manuscripts, the traditions I collected at Parima, and the hieroglyphics I saw in the desert of Casiquiare, where scarcely a vestige of a human being is now to be seen, added to what Clavigero has said of the emigration of the Mexicans toward the south, have suggested to me ideas respecting the origin of this people, which I shall pursue when I have leisure.

Nevado del Atlas, once the highest mountain in the world.

Hieroglyphics.

American languages not poor.

I have likewise paid much attention to the study of the American languages, and found what Condamine has said of their poverty to be extremely false. The Caribbee is rich, beautiful, energetic, and polished: it is not destitute of expressions for abstract ideas; and it has numerical terms sufficient for any possible combination of figures. The Inca is particularly rich in delicacy and variety of expression. The priests knew how to draw a meridian line, and observe the solstices: they had reduced the lunar to a solar year by intercalations: and the savages even at Erevato, in the interior of Parima, believe the moon to be inhabited, and know, from the traditions of their ancestors, that its light is derived from the sun.

Caribee.

Inca.

Ancient science.

Crocodile increases air by respiration.

At Monpox I made some very curious experiments on the respiration of the crocodile, having procured forty or fifty young ones. Instead of diminishing the quantity of the air in which it respires like other animals, the crocodile increases it. A crocodile placed in 1000 parts of atmospheric air, consisting of 274 oxigen, 15 carbonic acid, and 711 azot, increased it in an hour and forty-three minutes, by the addition of 124 parts. The carbonic acid had received an augmentation of 64 parts: the oxigen had been diminished 167; but, as 46 are contained in the carbonic acid produced, the crocodile had appropriated to itself only 121 parts, a small quantity considering the colour of its blood: and 227 parts of azot, or other gasses, on which acidifiable

Air 274 oxigen, 15 carbonic acid, 711 azot.

bases

bafes had no action, were produced. For the analyfis I ufed lime water and nitrous gas, prepared with great care.

Near Santa Fee, at an elevation of 2890 yards, are found an immenfe number of foffil bones of elephants, both of the African fpecies and of the carnivorous kind, difcovered in North America. I have fince received others from a part of the Andes, about the latitude of 2° from Quito, and from Chili: fo that thefe animals muft have exifted from Patagonia to the Ohio.

Large foffil bones at a confiderable height, and nearly from one extremity of America to the other.

VIII.

Method of meafuring any Aliquot Part of an Inch by a Screw, which gives no fuch Part in its Turn; and Obfervation on an Error of Edwards in placing the Eye Stop of reflecting Telefcopes. In a Letter from Mr. J. C. HORNBLOWER.

To Mr. NICHOLSON.

DEAR SIR,

BEING on a vifit about fifteen years ago in the confines of the principality, (a region fcience never yet explored) I was in want of a reticulated fquare to be placed in the focus of the eye glafs of an optical inftrument, and by the difappointment ufually attending jobs of this kind when done at a diftance, I refolved on accomplifhing it myfelf, though I forefaw it would be a difficult undertaking, efpecially as I am but an indifferent workman even with the beft tools, and with fuch as lay before me, I could not anticipate much pleafure in my tafk.

Conftruction of a reticulated fquare.

The firft thing which occurred to me was, the conftruction of the old fafhioned micrometer, and I found a tap which promifed well for the purpofe; but unfortunately it had no determinate number of threads; but neverthelefs I was refolved to proceed, and by this tap I cut a pair of dies, and by them I cut a fcrew on a piece of large brafs wire, which being more homogenous than fteel in general, had a better chance of the accuracy my inftrument required, however I fet the nut in a jimbal, and on trying the fcrew, I found it had 26,6 threads in an inch, and as I wanted tenths of an inch, I muft neceffarily provide means to correct the excefs or defect in the fcrew, which I did thus:

A fcrew was made by tapping.

The

This was fitted up with a micrometer plate, and had a clip, by which the screw could be held fast while the plate was shifted.

The ſcrew had a collar, which I could draw together ſo as to hold it faſt, and another collar, on which I put the divided plate, and after that an index firmly fixed on the ſcrew. The plate alſo having liberty to turn on its collar, I could faſten it independent of the ſcrew, by having two thumb ſcrews oppoſite each other, bearing on the periphery of the plate, ſo that I could turn the ſcrew only, or turn the plate only, or turn them all together.

The ſcrew was turned, the quantity anſwering to 0.1 inch; then clipped faſt and the micrometer ſet to zero: the ſcrew was ſet free and moved as before; then clipped, and the micrometer ſet; the ſcrew then ſet free and moved, &c. &c.

The operation was as follows: Having laid the frame I had to divide on the board, to which this apparatus was connected, I brought the index to 0 on the plate, and made it (the plate) faſt, then turned the index two revolutions and ,66 of a revolution: This gave me a diviſion of ,1 inch. I then faſtened the ſcrew by bringing the collar to bind upon it; liberated the plate, and turned it until 0 on the diviſions came to the index. The plate was then faſtened, and the ſcrew ſet free and turned 2,66 revolutions as before. The ſcrew was then again faſtened and the plate ſet free, and 0 brought to the index as before, after which the plate was faſtened, and the ſcrew ſet free and turned, &c. With a little addition of apparatus to make the faſtenings and looſenings inſtantaneous; this method may be uſed to much advantage when a more elaborate or ſcientific inſtrument cannot be obtained.

I forgot in our converſation on the ſubject of reflecting teleſcopes, to bring to your mind a falſe calculation of Mr. Edwards's in his directions for making ſpecula for reflecting teleſcopes, publiſhed in the nautical almanack ſome years ſince.

Mr. Edwards aſſerts that the eye ſtop of a reflecting teleſcope ought to be in the focus of the eye glaſs.

Concerning the place of the eye ſtop, his words are "It is abſolutely neceſſary for perfect viſion that the eye ſhould be applied to a ſmall hole of a certain dimenſion, placed exactly in the focus of the ſingle eye-glaſs, if the eye-piece conſiſt of one glaſs only, or elſe in the compound focus of the glaſſes; if it is conſtructed with two, as is moſt commonly practiſed;" and afterwards ſays, "Let the diſtance of the eye-hole from the eye-glaſs, if it is a ſingle one, be put as near as can be attained by meaſure, equal to the focal diſtance of the eye-glaſs, &c."

But it ought not to be at the focus of parallel rays; but of rays from the vortex of the ſmaller ſpeculum.

Surely Mr. Edwards muſt have taken this on a bare ſuppoſition, without ever enquiring why. When we ſpeak of the focus of a lens, it is generally underſtood to mean the focus of parallel rays, but the focus of parallel rays does not determine

mine the place of the eye ſtop, but a focus, where the image of the ſmall ſpeculum is formed, and this will be more or leſs accordingly as the diſtance between the lens and the ſpeculum is leſs or more. For inſtance, take a lens of 1,5 inch focus, which would ſuit a ſpeculum of 9 inches focus, and the diſtance of the ſmall ſpeculum will be about 14 inches from the eye glaſs. Then $\frac{14 \times 1,5}{14 - 1,5} = 1,68$ will be the true diſtance of the eye ſtop, or in other words, the focus of rays proceeding from an object placed 14 inches beyond the lens, almoſt two inches farther than the ſolar focus.

I am,

DEAR SIR,

Your moſt obedient Servant,

J. C. HORNBLOWER.

IX.

Account of a new Apparatus conſtructed for the Purpoſe of meaſuring the Elaſtic Force, and regulating the Emiſſion of Steam from the Boiler in which it is generated. Communicated by the Inventor, MR. ARTHUR WOOLF, *Engineer.*

PLATE XIV. exhibits a meaſured ſection of the ſelf-acting and regulating ſteam valve. A A repreſents the upper part of the boiler, having its mouth or neck cylindrical. and cloſed by a well-fitted, but eaſily moving valve plug B B C C, which is in fact a metallic tube, open at bottom and cloſed above, by a cap-piece B B, that by its chamfered rim or projecting part affords the accurate valve-cloſure when down. The interior parallel lines at D ſhew the place where a long perforation is made through the ſide of the cylindrical part of the valve plug from its cap, nearly down to the bottom; which perforation affords a paſſage for the ſteam, increaſing in magnitude as the elaſtic force cauſes the valve to riſe. E is the ſide paſſage for conveying the ſteam to its place of operation. F is the rod or tail of the valve paſſing through a ſtuffing box above, and attached by a chain to the ſector G, and by its means moving the lever that carries the ball H.

Self acting and regulating ſteam valve.

The

The above conftitutes the whole of this fimple and effectual contrivance, and its mode of operation fcarcely needs to be defcribed. As the fteam becomes ftronger it raifes the valve, and efcapes through D, and raifes the weight H higher the more the preffure within exceeds that of the working fteam in the upper fpace F E.

X.

Journey to the Summit of Mont-Perdu. By Cit. RAMOND *.

Sauffure's travels in the Alps highly ufeful to geology.

THE many excellent obfervations made by the celebrated Sauffure in the Alps, traverfing that grand affemblage of mountains in all directions, have contributed more effectually to the advancement of geology, than all the hypothefes that have been formed.

Ramond's in the Pyrenees.

Cit. Ramond is rendering a fimilar fervice to the fcience, by his repeated journies in the Pyrenees; and his adventurous refearches will foon bring us acquainted with a great part of that chain, the ftructure of which is fo different from that of the Alps. In a work publifhed two years ago, he defcribed the bafis of Mont-Perdu: he had even approached its fummit, and had obferved that this mountain, the loftieft of the Pyrenees, was calcareous, and contained fhells and other organized bodies, in a foffil ftate, at an elevation of about 3600 metres.

Reached the fummit of Mont Perdu.

Vertical ftrata of carbonated lime.

Horizontal of calcareous fandftone.

In the journey he made in Auguft 1802, he reached the fummit of the mountain by paffing the Col de Fanlo, or Nifcle. In this road he conftantly found ftrata of compact carbonated lime in a pofition nearly vertical. They include ftrata of calcareous fandftone, and thefe ftrata fometimes cover the falient angles of the vertical ftrata, nearly in a horizontal direction. This calcareous ftone falls off fpontaneoufly in little irregular fragments: on the flighteft friction it emits a naufeous fetid fmell. Some of the ftrata of this ftone contain nodules of flint; others fuch confiderable maffes of *camerines,* that the ftone appears entirely compofed of them.

Summit a fetid limeftone.

The fummit is formed of a fetid lime-ftone, contaminated with quartz, and containing a little iron, and $\frac{1}{310}$ of carbon, without alumine. Cit. Ramond found no fragments of fhells: but this ftone being of a fimilar

* Bulletin des Sciences.

kind to the neighbouring ſtrata, in which they occur, he is inclined to believe, that he ſhould diſcover ſome on a more ſedulous reſearch. The elevation of this ſummit is the ſame as that of the Col du Géant in the Alps, or 3426 metres.

Its height 3727 yards.

From this, the loftieſt point of the Pyrenean chain, Cit. Ramond could more eaſily obſerve the general form of the whole. Looking toward France, the chain is broad, and formed of ſeveral parallel lines of mountains, in the midſt of which are ſeen the lines of granite and gneiſs, of which the peak of Bagneres is a part. Theſe are more diſtinguiſhable by their ſummits being rugged with aſperities, than for their elevation. Theſe lines imperceptibly diminiſh in height till they reach the plain, which is too far diſtant to be ſeen. To the ſouth the appearance is very different. The whole declines ſuddenly, and at once. It is a precipice of ten or eleven hundred metres, the bottom of which is the ſummit of the higheſt mountains of this part of Spain. Not one of them, however, has 2500 metres abſolute height, and they ſoon ſink into low rounded hills, beyond which is the vaſt proſpect of the plains of Arragon.

Parallel lines of mountains on the French ſide

Granite and gneiſs in the middle.

On the Spaniſh ſide a lofty precipice:

From the ſummit of Mont-Perdu, on the Spaniſh ſide, is ſeen a vaſt flat of limeſtone, the ſurface of which, from that elevation, appears almoſt ſmooth. This flat is interſected by four or five vaſt chaſms with perpendicular ſides, the angles and ſinuoſities of which correſpond to each other with aſtoniſhing exactneſs. Theſe broad and deep chaſms diverge from the baſe of the peak, and their bottoms are covered with thick woods. There is no way of entering them but at their mouths. Cit. Ramond proceeded by the way of Val de Broto, and entered that called by the natives Val d'Ordeſa. It is a deep valley, uninhabited, and bordered by ſteep walls about 896 metres high. Theſe you can aſcend only in few places, and with the greateſt difficulty. You then reach the flat. The ſtrata that form it, and in which theſe vaſt chaſms have been opened, are horizontal, or very little inclined. They conſiſt of red ſandſtone of ancient formation, pudding-ſtone, and compact limeſtone. All theſe ſtones are diſpoſed to break off in a direction perpendicular to their beds, and Cit. Ramond aſcribes this diſpoſition to the quartz they contain. He thinks that the chaſms, opened at firſt by ſome unknown cauſe, have been enlarged by the crumbling of their ſides in this manner.

Beneath this a flat of limeſtone, interſected by vaſt chaſms.

Val d'Ordeſa.

The ſtrata red ſandſtone, pudding-ſtone, and limeſtone.

On

The peaks on the two fides rife in oppofite directions.

On approaching the peaks that rife from this flat, the ftrata, which are of compact fhelly limeftone, rife at an angle of 45°, but in a direction contrary to that of the ftrata that form the fmall peaks on the northern, or French fide. Thus thefe ftrata as they rife diverge like the fticks of an opened fan, the vertical ones conftitute the fummits; a remarkable arrangement, which Cit. Ramond afcribes to a fliding of the ftrata, rather than a rifing up, properly fo called, from a depreffion of the other end.

Cit. Ramond has afcertained the limits of permanent fnow, and of vegetation, for this lofty part of the Pyrenean chain.

Limits of permanent fnow, and of vegetation.

The fnow terminates at 2440 metres. The laft trees are Scotch firs, which reach 2150 metres. Next come the fhrubs, of which the juniper is the higheft. At 2760 metres are found the *ranunculus parnaffia-folius*, the *faxifraga Groenlandica*, &c. then the *artemifia rupeftris* of Lamarck; and laftly, round the very peak of Mont-Perdu. on rocks too floping to retain the fnow, grow the *ceraftium*, perhaps the *alpinum* of Linneus, and the rofe-flowering *aretia alpina*.

XI.

*Notice of a Method of giving the Appearance of Cotton to Hemp or Flax**. *By Cit.* Berthollet.

The author's former experi-

WHILE I was engaged in the application of the oxigenated muriatic acid, to the art of bleaching, I made experiments on flax, and I inferted this obfervation in the firft volume of *Elemens de Tainture*, p. 258. "I have endeavoured to bleach flax completely, by the method I make ufe of with thread; but although its filaments may not lofe much of their folidity by this, they neverthelefs acquire fuch a tendency to feparate and divide themfelves, as renders them much more difficult to fpin, and they form a thread of much lefs folidity."

Subfequent attempts.

Since that period different artifts have employed themfelves, with various fuccefs, in methods of obtaining from flax a matter analogous to cotton. A Swifs, Cit. Clays has even formed an eftablifhment fome time ago, in which this kind of preparation is executed.

* From Journal de L'Ecole Polytechnique, Tome IV, p. 319.

I know

I know not the proceffes which have been heretofore employed, but I have fucceeded, by means of the oxigenated muriatic acid, in obtaining a matter more beautiful than any of thofe, the knowledge of which has reached me.

The very fimple procefs I am about to defcribe, was executed in the laboratory of the Polytechnic School, by Cit. Gai-Luffac, at that time one of it pupils.

The flax is cut into fragments about fix centimetres long; it is covered with water in which it is left for three or four days; after this it is boiled in fimple water; it is wafhed with care; it is lyed, it is put into oxigenated muriatic acid. Four immerfions in the oxigenated muriatic acid and four lyes are commonly fufficient: the operation is finifhed by immerfing it in a bath of water charged with one two-hundredth part of fulphuric acid. On removing it from this tepid bath in which it has been left for near half an hour, it is wafhed with great care, and plunged into water charged with foap: it is then fpread, without being wrung, on hurdles, where it is left to dry, without, however, fuffering it to become too dry. All thefe operations, from the firft immerfion to the drying, do not require more than five or fix hours, when the procefs is made with fmall quantities. Berthollet's procefs.

The flax thus prepared was then fent to Cit. Molar, who was kind enough to undertake the mechanical operations: he firft combed and then carded the bleached flax. He experienced fome difficulties from the knots that were fcattered through the flax, but this fkilful mechanic foon overcame this inconvenience. I prefented to the clafs of phyfical and mathematical fciences of the Inftitute, on the 6th Prairial of the year 8, a fample of the prepared materials, which was equal to cotton in its whitenefs, and other apparent qualities; neverthelefs, Cit. Molar found fault with the cottony matter for being too fhort in the ftaple. Mechanical operations. The filaments are too fhort.

Cit. Bawens alfo manufactured the cottony matter prepared in the laboratory of the fchool, with the beautiful machines which he poffeffes at his manufactory at Chaillot. He found no difficulty in the execution, but he alfo found the filaments too fhort, although he procured a very fine thread of confiderable tenacity.

It is therefore the inconvenience of being reduced into fhort filaments which requires to be corrected in the firft preparation; Propofed remedy.

preparation; and I am of opinion one certain method of accomplishing it, is not to complete the bleaching, but to stop at the third operation. If four are required for the thorough bleaching, it must then be finished in the thread or in the stuff.

Directions for the bleaching.

In the operation of bleaching, too strong lyes must be avoided, but they must be made use of in boiling. We are convinced that all the means which diminish the odour of the oxigenated muriatic acid weaken its action; hence it must be employed in a state of purity, and we must not attempt to preserve ourselves from its odour, but by the construction of the apparatus and the mode of application, objects which use has rendered easy: it must even be used in its concentrated state, otherwise the operations require to be much increased.

The process was finished by immersion in water charged with soap, which was not pressed out, in order that the filaments might not adhere too much by drying, but yield easily the separation which is to be performed by the card. But there is a probability that by preventing too much drying, the inconvenience experienced in the first trials would not take place, and that this immersion might then be omitted.

The cottony matter is obtained equally good from fine flax or coarse hemp.

It is remarkable that whether the finest flax or the coarsest hemp is made use of, the filaments obtained are of equal fineness and colour.

This indication will be guide enough to artists, well acquainted with chemical manipulations, in the operation of bleaching. But I have nothing to say on the mechanical dispositions of carding and spinning, because they were not executed by me.

Probable advantages to be obtained by the process.

If I am not deceived, this application of a process already old, offers many advantages, because it may change the fabrication of thread, which, to this day, requires the spinning-wheel into that much less expensive, which is executed by means of machinery; and it may convert a rough product of our agriculture, and even some of the refuse, such as that from rope-walks, into a substance valuable in the arts. This motive has induced me to insert this notice in the Journal of an establishment devoted to public utility, although it offers nothing new as a scientific subject.

XII. *Experimental*

XII.

Defcription of a Machine now in actual and daily Ufe, for cleanfing Chimnies, without the Affiftance of Climbing-boys, and with much greater Effect than is produced by that Method. Communicated by the Inventor, Mr. CHRISTOPHER SMART, *of Ordnance Wharf, Weftminfter Bridge.* W. N.

EVERY humane perfon muft have beheld with pain and regret, the infant victims of a filthy and difgufting operation, who are expofed to daily fuffering, and too often to permanent difeafe and decrepitude, without the hope of fubfiftence when grown to manhood. A remedy for the cafe of thefe unhappy and devoted children was long ago attempted by the amiable and benevolent Jonas Hanway; and within the laft twelve months, another philanthrophift, James Hebdin, efq; has actively exerted himfelf to form a fociety for promoting and eftablifhing methods of cleaning chimnies more worthy of a great and civilized people, than one grounded on the mifery of the weak and the helplefs. My gratulations, and thofe of every good man, will form but a fmall portion of that reward which the internal confcioufnefs of the extenfive good he has done muft afford. I fhall therefore difmifs any farther confideration of the perfonal merits of thofe by whom the attention of mechanics has been directed to this object, or of the artifts who have laboured to folve the problem offered to their ingenuity; and fhall proceed to defcribe Mr. Smart's machine. This apparatus has been approved upon trial by the fociety lately eftablifhed, and has anfwered to the fatisfaction of feveral well qualified employers, whofe certificates I have feen, by bringing down more than the ufual quantity of foot, as well as by its efficacy in lofty or winding chimnies, and fuch as are too narrow to be fwept by children. It may eafily be inferred, that it muft be ftill more advantageous in chimnies on fire, than the fhocking procefs of fending up a child wrapped in rags to enter an actual place of combuftion and fuffocating vapour.

Machine for cleanfing chimnies.

Plate XIII. *Fig.* 1. reprefents an apparatus of brufhes, fuppofe four, which are fixed by hinges to a middle piece or bar, to

Machine for clearing chimnies.

fo that they fhall be capable either of hanging down, parallel to the bar, or of being opened and expanded, fomewhat in the manner of an umbrella, until they ftand out at right angles with the middle piece; in which fituation they are retained by fmall fupporting bars, refembling thofe of the fame well-known utenfil. *Fig.* 2. fhews the brufhes in their collapfed ftate, with an appendage of tubes, by which the fyftem is thruft up the chimney. A ftrong cord is paffed through a feries of thefe tubes, the lower mouth of every one of which is opened a little, in order to admit the upper ends of each in fucceffion. *Fig.* 5. fhews the fweeping man employed, thrufting the apparatus up the chimney; in which the fet of connected tubes forms a piece, having enough of flexibility to accommodate itfelf to the chimney, and yet fufficiently rigid to anfwer the purpofe of carrying up the fet of brufhes. When thefe have paffed through the chimney-pot, and given the ufual evidence of the work being to be performed from one end of the chimney to the other, the rope is drawn tight in order to fet the tubes fteadily together, and then fecured by a thumb fcrew feen in *Fig.* 2.—after which, the fweeper begins to pull it downwards. The rim of the narrow opening of the chimney-pot caufes the brufhes to expand, and in this ftate they are retained by the ufual fpring-catch feen in *Fig.* 1. and by the fimple and gradual defcent, the chimney becomes effectually cleared of its foot.

Fig. 3. reprefents a curtain for defending the apartment againft the foot. It is fupported by a rod of metal, having a cork fixed or ftuck in one of its ends, to afford a fpringy and perfectly harmlefs bearing againft the infide lining of the chimney-piece, whether of marble or any other material, and the clamp which is feen towards the other end of the rod, has likewife a facing of cork, and is fixed at any diftance, fo as to afford the oppofite bearing. The fides of the curtain are fecured by rods, *Fig.* 4. which can be lengthened or fhortened by two parts fliding together, as is feen in the meafuring rule of fhoemakers, or perhaps more familiarly, in thofe fliding pencils, which have now been feveral years in ufe.

I have been affured that the cleanlinefs, decency, and quiet operation of this engine, are by no means among the fmalleft of its recommendations. I have not yet had an occafion of trying it in my own houfe, but fhall certainly do it

on

on the first opportunity, and shall then either confirm or modify in a future notice, as my own observation shall direct, these particulars of information, which upon good grounds I have thought myself justified in now laying before my readers.

W. N.

XIII.

Experimental Essays on the Constitution of mixed Gases; on the Force of Steam or Vapour from Water and other Liquids in different Temperatures, both in a Torricellian Vacuum and in Air; on Evaporation; and on the Expansion of Gases by Heat. By JOHN DALTON *.

THE progress of philosophical knowledge is advanced by the discovery of new and important facts; but much more when those facts lead to the establishment of *general* laws. It is of importance to understand that the descent of falling bodies is the same every where on the surface of the earth; but from that and some other particular facts to infer the law of gravitation, or that all matter attracts with a force decreasing as the square of the distance, is a much higher attainment in science. In the train of experiments lately engaging my attention some new facts have been ascertained, which with others, seem to authorize the deduction of general laws, and such as will have influence in various departments of natural philosophy and chemistry.

The discovery of general laws is of high value in the advancement of science.

* These interesting treatises were read before the literary and philosophical Society at Manchester, in October, 1801, and are published in the fifth volume of their memoirs. The first, on mixed gases, was communicated in the same month, in a somewhat different form, by the author to our Journal, and published in the quarto series, vol. V. p. 241.—and, a further communication from him on the same subject appeared in vol. III. p. 267 of our present series. I have not, therefore, reprinted that essay of the present collection. The last essay in the title, viz. on the expansion of gases, is inserted in the last mentioned volume, p. 130.—Consequently though the title and introduction refer to the whole four; yet the present article contains only what was wanting to complete the readers possession of this valuable mass of experimental knowledge; that is to say, the essays upon steam and upon evaporation.

Statement of certain laws previous to their fundamental experiments.

As the detail of experiments will be best understood and their application seen, if the laws of principles alluded to be kept in view, it may be proper here to state them; though it must not be understood that they were proceeded upon hypothetically in the direction of those experiments. On the contrary, the first law, which is as a mirror in which all the experiments are best viewed, was *last* detected, and after all the particular facts had been previously ascertained.

1. Mixed elastic fluids do not repel each other.

1. When two elastic fluids, denoted by *A* and *B*, are mixed together, there is no mutual repulsion amongst their particles; that is, the particles of *A* do not repel those of *B*, as they do one another. Consequently, the pressure or whole weight upon any one particle arises solely from those of its own kind.

2. The steam of any liquid, at any given number of degrees from its boiling point, acts the same as that of any other liquid at the same number of degrees, the same way from its boiling point.

2. The force of steam from all liquids is the same, at equal distances above or below the several temperatures at which they boil in the open air: and that force is the same under any pressure of an other elastic fluid as it is in vacuo. Thus, the force of *aqueous* vapour of 212° is equal to 30 inches of mercury; at 30° below, or 182°, it is of half that force; and at 40° above, or 252°, it is of double the force; so likewise the vapour from sulphuric ether which boils at 102°, then supporting 30 inches of mercury, at 30° below that temperature it has half the force, and at 40° above it, double the force: and so in other liquids. Moreover, the force of aqueous vapour of 60° is nearly equal to half inch of mercury, when admitted into a torricellian vacuum; and water of the same temperature, confined with perfectly dry air, increases the elasticity to just the same amount.

3. Evaporation at any temperature is as the force of the steam.

3. The quantity of any liquid evaporated in the open air is directly as the force of steam from such liquid at its temperature, all other circumstances being the same.

4. All elastic fluids expand equally by heat.

4. All elastic fluids expand the same quantity by heat: and this expansion is very nearly in the same equable way as that of mercury; at least from 32° to 212°.—It seems probable the expansion of each particle of the same fluid, or its sphere of influence, is directly as the quantity of heat combined with it; and consequently the expansion of the fluid as the cube of the temperature, reckoned from the point of total privation.

Having

Having now ftated the chief principles which feem to be eftablifhed from the following feries of facts and obfervations, I fhall proceed to treat of them under the feveral heads *.

ESSAY II.

On the Force of Steam or Vapour from Water and various other Liquids, both in a Vacuum and in Air.

SECTION I.—*On Vapour in Vacuo.*

THE term *fteam* or *vapour* is equally applied to thofe elaftic fluids which, by cold and preffure of certain known degrees, are reduced wholly or in part into a liquid ftate. Such are the elaftic fluids arifing from water, alkohol, ether, ammonia, mercury, &c. Other elaftic fluids that cannot be reduced, or rather that have not yet been reduced, into a liquid ftate by the united agency of thofe two powers, are commonly denominated *gafes*. There can fcarcely be a doubt entertained refpecting the reducibility of all elaftic fluids of whatever kind into liquids; and we ought not to defpair of effecting it in low temperatures and by ftrong preffure exerted upon the unmixed gafes. However uneffential the diftinction between the gafes and vapours may be in a chemical fenfe, their *mechanical* action is very different. By increafing the quantity of any gas in a given fpace the force of it is proportionally increafed; but increafing the quantity of any liquid in a given fpace does not at all affect the force of the vapour arifing from it. On the other hand, by increafing the temperature of any gas a proportionate increafe of elafticity enfues; but when the temperature of a liquid is increafed, the force of vapour from it is increafed with amazing rapidity, the increments of elafticity forming a kind of geometrical progreffion, to the arithmetical increments of heat.—Thus, the ratio of the elaftic force of atmofpheric air of 32° to that at 212°, is nearly as 5 : 7; but the ratio of the force of aqueous vapour proceeding from water of 32° and 212°, is as 1 : 150 nearly.

Steam or vapour defined. It is an elaftic fluid capable of becoming liquid by cold and preffure. Gafes not fo.

Remarkable difference between the expanfion of fteam and of gas by heat: the former being prodigioufly greater.

The object of the prefent effay is to determine the utmoft force that certain vapours, as that from water, can exert at different temperatures. The importance hitherto attached to this enquiry has arifen chiefly from the confideration of fteam

Object of the prefent effay.

* Here followed the effay I. on mixed gafes. W. N.

as a mechanical agent; and this has directed the attention more especially to high temperatures. But it will appear from what follows that the progress of philosophy is more immediately interested in accurate observations on the force of steam in low temperatures. Different authors have published accounts of their experiments on the force of steam: I have on a former occasion (Meteorological Essays, page 134) given a table of forces for every 10° from 80° to 212°. The author of the article "Steam," in the Encyclopedia Britannica, has done the same from 32° to 280°: and M. Betancourt, in the "Memoirs des scavans etrangeres" for 1790, see Hutton's Math. Diction. page 755) has given tables on the subject, both for vapour from water and spirit of wine, also from 32° to 280°. But these two authors, having assumed the force of vapour from water of 32° to be nothing, are essentially wrong at that point and in all the lower parts of the scale; and in the higher part, or that above 212°, they determine the force too much: owing as I apprehend to a quantity of air, which being disengaged from the water by heat and mixing with the steam, increases the elasticity.—In a question of such moment it seemed therefore desirable to obtain greater accuracy.

Reference to authors respecting steam.

Encyl. Britt.

Betancourt.

My method is this: I take a barometer tube perfectly dry, and fill it with mercury just boiled, marking the place where it is stationary; then having graduated the tube into inches and tenths by means of a file, I pour a little water (or any other liquid the subject of experiment) into it, so as to moisten the whole inside; after this I again pour in mercury, and, carefully inverting the tube, exclude all air: the barometer by standing some time exhibits a portion of water, &c. of $\frac{1}{8}$ or $\frac{1}{10}$ of an inch upon the top of the mercurial column; because being lighter it ascends by the side of the tube; which may now be inclined and the mercury will rise to the top manifesting a perfect vacuum from air. I next take a cylindrical glass tube open at both ends, of 2 inches diameter and 14 inches in length; to each end of which a cork is adapted, perforated in the middle so as to admit the barometer tube to be pushed through and to be held fast by them; the upper cork is fixed two or three inches below the top of the tube and is half cut away so as to admit water, &c. to pass by; its service being merely to keep the tube steady. Things being thus circumstanced

The author's method. A minute portion of the fluid is put into the upper space of a barometer. Heat is applied by the external contact of water. The fall of the mercury shews the effect.

ſtanced, water of any temperature may be poured into the wide tube, and thus made to ſurround the upper part or vacuum of the barometer, and the effect of temperature in the production of vapour within can be obſerved from the depreſſion of the mercurial column. In this way I have had water as high as 155° ſurrounding the vacuum: but as the higher temperatures might endanger a glaſs apparatus; inſtead of it I uſed the following:—

The veſſel containing the external water was of tin for temperatures near boiling; and a ſyphon barometer was uſed to ſhew the depreſſion.

Having procured a tin tube of four inches in diameter and two feet long, with a circular plate of the ſame ſoldered to one end having a round tube in the center like the tube of a reflecting teleſcope, I got another ſmaller tube of the ſame length ſoldered into the larger, ſo as to be in the axis or centre of it: the ſmall tube was open at both ends, and on this conſtruction water could be poured into the large veſſel to fill it, whilſt the central tube was expoſed to its temperature. Into this central tube I could inſert the upper half of a ſyphon barometer, and fix it by a cork, the top of the narrow tube alſo being corked; thus the effect of any temperature under 212° could be aſcertained, the depreſſion of the mercurial column being known by the aſcent in the exterior leg of the ſyphon.

Another method by obſerving the boiling point in the air pump, and the ſtation of the barometer gage.

The force of vapour from water between 80° and 212° may alſo be determined by means of an air-pump; and the reſults exactly agree with thoſe determined as above. Take a Florence flaſk half filled with hot water, into which inſert the bulb of a thermometer; then cover the whole with a receiver on one of the pump-plates, and place a barometer gage on the other: the air being ſlowly exhauſted, mark both the thermometer and barometer at the moment ebullition commences, and the height of the barometer gage will denote the force of vapour from water of the obſerved temperature. This method may alſo be uſed for other liquids. It may be proper to obſerve the various thermometers uſed in theſe experiments were duly adjuſted to a good ſtandard one.

Hence the force of ſteam up to 212° was had.

After repeated experiments by all theſe methods, and a careful compariſon of the reſults, I was enabled to digeſt the following table of the force of ſteam from water in all the temperatures from 32° to 212°.

Two important enquiries ſtill remained, the firſt to determine the force of ſteam from water above 212° and below

 32°;

32°; the fecond, to determine the comparative forces of vapour from other liquids. Thefe enquiries feemed independent of each other; notwithftanding which I found them in reality connected.

Examination of the progreffion of the force of vapour.

Upon examination of the numbers in the table, within the limits juft mentioned, there appears fomething like a geometrical progreffion in the forces of vapour; the ratio, however, inftead of being conftant, is a gradually diminifhing one: thus the

			Ratios.
Force at	32° =	,200 inch.	
			17. 50
	122 =	3. 500	
			8. 57
	212 =	30. 000	

If we divide thefe ratios, according to obfervation, they will ftand thus:

			Ratios.
Force at	32° =	,200 inch.	
			4. 550
	77 =	,910	
			3. 846
	122 =	3. 500	
			3. 214
	167 =	11. 250	
			2. 666
	212 =	30. 000	

If we divide thefe again, they become,

			Ratios.
Force at	32° =	,200 inch.	
			2. 17
	$54\frac{1}{2}$ =	,435	
			2. 09
	77 =	,910	
			2. 00
	$99\frac{1}{2}$ =	1. 820	
			1. 92
	122 =	3. 500	
			1. 84
	$144\frac{1}{2}$ =	6. 450	
			1. 75
	167 =	11. 250	
			1. 67
	$189\frac{1}{2}$ =	18. 800	
			1. 59
	212 =	30. 000	

By

Examination of the progreſſion of the force of vapour.

By another diviſion we obtain the ratios for every 11¼° of temperature from 32° to 212°, as under:

Force at 32°	=	,200 inch	
			1. 485
43¼	=	,297	
			1. 465
54½	=	,435	
			1. 45
65¾	=	,630	
			1. 44
77	=	,910	
			1. 43
88¼	=	1. 290	
			1. 41
99½	=	1. 820	
			1. 40
110¾	=	2. 510	
			1. 38
122	=	3. 500	Ratios,
			1. 36
133¼	=	4. 760	
			1. 35
144½	=	6. 450	
			1. 33
155¾	=	8. 550	
			1. 32
167	=	11. 250	
			1. 30
178¼	=	14. 600	
			1. 29
189½	=	18. 800	
			1. 27
200¾	=	24. 000	
			1. 25
212	=	30. 000	

The ratio of augmentation in the force of vapour is not as the degrees, but gradually leſs.

Thus it appears that a ratio having a uniform decreaſe nearly takes place; and we may therefore extend the table of forces at both extremes, without the aid of experiment, to a conſiderable diſtance. Thus aſſuming the ratios for each interval of a 11°¼ below 32° to be, 1.500, 1.515, 1.530, 1.545, &c. and for each interval above 212° to be 1.235, 1.220, 1.205, 1.190, 1.175, 1.160, 1.145, 1.130, &c. we can extend the table many intervals of temperature, and determine all the intermediate degrees by interpolation. This method may be relied upon as a near approximation;

however it does not fuperfede the expediency of determination by experiment; though that is much more difficult above 212°, and below 32°, than in the intermediate degrees; becaufe it is difficult to procure a fteady heat above 212°; and below 32° the variation of force becomes fo fmall as to elude minute difcrimination. It will appear from what follows that the extenfion of the table by this method above 212° is in all probability accurate, or very nearly fo, for 100° or more.

Table of the force of aqueous vapour or fteam.

TABLE

Of the Force of Vapour from Water in every temperature from that of the congelation of Mercury, or 40° below zero of Fahrenheit, to 325°.

Temperature.	Force of Vap. in Inches of Mercury.	Temperature.	Force of Vap. in Inches of Mercury.	Temperature.	Force of Vap. in Inches of Mercury.
-40°	,013	25°	,156	54°	,429
-30	,020	26	,162	55	,443
-20	,030	27	,168	56	,458
-10	,043	28	,174	57	,474
—	——	29	,180	58	,490
0	,064	30	,186	59	,507
1	,066	31	,193	60	,524
2	,068	—	——	61	,542
3	,071	32	,200	62	,560
4	,074	33	,207	63	,578
5	,076	34	,214	64	,597
6	,079	35	,221	65	,616
7	,082	36	,229	66	,635
8	,085	37	,237	67	,655
9	,087	38	,245	68	,676
10	,090	39	,254	69	,698
11	,093	40	,263	70	,721
12	,096	41	,273	71	,745
13	,100	42	,283	72	,770
14	,104	43	,294	73	,796
15	,108	44	,305	74	,823
16	,112	45	,316	75	,851
17	,116	46	,328	76	,880
18	,120	47	,339	77	,910
19	,124	48	,351	78	,940
20	,129	49	,363	79	,971
21	,134	50	,375	80	1. 00
22	,139	51	,388	81	1. 04
23	,144	52	,401	82	1. 07
24	,150	53	,415	83	1. 10

Tem-

Table continued.

Table of the force of aqueous vapor or steam.

Temperature.	Force of Vap. in Inches of Mercury.	Temperature.	Force of Vap. in Inches of Mercury.	Temperature.	Force of Vap. in Inches of Mercury.
84°	1. 14	128°	4. 11	172°	12. 73
85	1. 17	129	4. 22	173	13. 02
86	1. 21	130	4. 34	174	13. 32
87	1. 24	131	4. 47	175	13. 62
88	1. 28	132	4. 60	176	13. 92
89	1. 32	133	4. 73	177	14. 22
90	1. 36	134	4. 86	178	14. 52
91	1. 40	135	5. 00	179	14. 83
92	1. 44	136	5. 14	180	15. 15
93	1. 48	137	5. 29	181	15. 50
94	1. 53	138	5. 44	182	15. 86
95	1. 58	139	5. 59	183	16. 23
96	1. 63	140	5. 74	184	16. 61
97	1. 68	141	5. 90	185	17. 00
98	1. 74	142	6. 05	186	17. 40
99	1. 80	143	6. 21	187	17. 80
100	1. 86	144	6. 37	188	18. 20
101	1. 92	145	6. 53	189	18. 60
102	1. 98	146	6. 70	190	19. 00
103	2. 04	147	6. 87	191	19. 42
104	2. 11	148	7. 05	192	19. 86
105	2. 18	149	7. 23	193	20. 32
106	2. 25	150	7. 42	194	20. 77
107	2. 32	151	7. 61	195	21. 22
108	2. 39	152	7. 81	196	21. 68
109	2. 46	153	8. 01	197	22. 13
110	2. 53	154	8. 20	198	22. 69
111	2. 60	155	8. 40	199	23. 16
112	2. 68	156	8. 60	200	23. 64
113	2. 76	157	8. 81	201	24. 12
114	2. 84	158	9. 02	202	24. 61
115	2. 92	159	9. 24	203	25. 10
116	3. 00	160	9. 46	204	25. 61
117	3. 08	161	9. 68	205	26. 13
118	3. 16	162	9. 91	206	26. 66
119	3. 25	163	10. 15	207	27. 20
120	3. 33	164	10. 41	208	27. 74
121	3. 42	165	10. 68	209	28. 29
122	3. 50	166	10. 96	210	28. 84
123	3. 59	167	11. 25	211	29. 41
124	3. 69	168	11. 54	212	30. 00
125	3. 79	169	11. 83	—	—
126	3. 89	170	12. 13	213	30. 60
127	4. 00	171	12. 43	214	31. 21

Tem-

Table of the force of aqueous vapor or steam.

Table continued.

Temperature.	Force of Vap. in inches of Mercury.	Temperature.	Force of Vap. in inches of Mercury.	Temperature.	Force of Vap. in inches of Mercury.
215°	31. 83	252°	60. 05	289°	98. 96
216	32. 46	253	61. 00	290	100. 12
217	33. 09	254	61. 92	291	101. 28
218	33. 72	255	62. 85	292	102. 45
219	34. 35	256	63. 76	293	103. 63
220	34. 99	257	64. 82	294	104. 80
221	35. 63	258	65. 78	295	105. 97
222	36. 25	259	66. 75	296	107. 14
223	36. 88	260	67. 73	297	108. 31
224	37. 53	261	68. 72	298	109. 48
225	38. 20	262	69. 72	299	110. 64
226	38. 89	263	70. 73	300	111. 81
227	39. 59	264	71. 74	301	112. 98
228	40. 30	265	72. 76	302	114. 15
229	41. 02	266	73. 77	303	115. 32
230	41. 75	267	74. 79	304	116. 50
231	42. 49	268	75. 80	305	117. 68
232	43. 24	269	76. 82	306	118. 86
233	44. 00	270	77. 85	307	120. 03
234	44. 78	271	78. 89	308	121. 20
235	45. 58	272	79. 94	309	122. 37
236	46. 39	273	80. 98	310	123. 53
237	47. 20	274	82. 01	311	124. 69
238	48. 02	275	83. 13	312	125. 85
239	48. 84	276	84. 35	313	127. 00
240	49. 67	277	85. 47	314	128. 15
241	50. 50	278	86. 50	315	129. 29
242	51. 34	279	87. 63	316	130. 43
243	52. 18	280	88. 75	317	131. 57
244	53. 03	281	89. 87	318	132. 72
245	53. 88	282	90. 99	319	133. 86
246	54. 68	283	92. 11	320	135. 00
247	55. 54	284	93. 23	321	136. 14
248	56. 42	285	94. 35	322	137. 28
249	57. 31	286	95. 48	323	138. 42
250	58. 21	287	96. 64	324	139. 56
251	59. 12	288	97. 80	325	140. 70

On Vapour from Ether, &c.

Vapor from ether and from other liquids follow a general law in their force.

We come now to the consideration of vapour from other liquids. Some liquids are known to be more evaporable than water; as liquid ammonia, ether, spirit of wine, &c. others less; as, quicksilver, sulphuric acid, liquid muriate of lime,

lime, ſolution of potaſh, &c. and it appears that the force of vapour from each in a vacuum is proportionate to its evaporability. M. Betancourt maintains that the force of vapour from ſpirit of wine is in a conſtant ratio to that from water at all temperatures; namely, as 7 to 3 nearly. My firſt experiments with ſpirits of wine led me to adopt this concluſion, and naturally ſuggeſted that the force of vapour from any other liquid would bear a conſtant ratio to that of water. The principle, however, is not true, either with regard to ſpirit of wine or any other liquid. Experiments made upon ſix different liquids agree in eſtabliſhing this as a general law; namely, *that the variation of the force of vapour from all liquids is the ſame for the ſame variation of temperature, reckoning from vapour of any given force:* thus aſſuming a force equal to thirty inches of mercury as the ſtandard, it being the force of vapour from any liquid boiling in the open air, we find *aqueous* vapour loſes half its force by a diminution of 30° degrees of temperature; ſo does the vapour of any other liquid loſe half its force by diminiſhing its temperature thirty degrees below that in which it boils; and the like for any other increment or decrement of heat. This being the caſe, it becomes unneceſſary to give diſtinct tables of the force of vapour from different liquids, as one and the ſame table is ſufficient for all. But it will be proper to relate the experiments on which this concluſion reſts.

The law enunciated.

Experiment on Sulphuric Ether.

The ether I uſed boiled in the open air at 102 .—I filled a barometer tube with mercury, moiſtened by agitation in ether. After a few minutes a portion of ether roſe to the top of the mercurial column, and the height of the column became ſtationary. When the whole had acquired the temperature of the air in the room, 62°, the mercury ſtood at 17.00 inches, the barometer at the ſame time being 29.75. Hence the force of vapour from ether at 62 is equal to 12.75 inches of mercury, which accords with the force of aqueous vapour at 172°, temperatures which are 40° from the reſpective boiling points of the liquids. By ſubſequent obſervations I found the forces of the vapour from ether in all the different temperatures from 33° to 102° exactly correſponded with the forces of aqueous vapour of the like range

Experiments with ether; in the barometer, below ebullition.

range, namely from 142° to 212°: the vapour from ether depreffes the mercury about fix inches in the temperature of 32°.

above ebullition

Finding that ether *below* the point of ebullition agreed with water below the faid point, I naturally concluded that ether *above* the point would give the fame force of vapour as water above it; and in this I was not difappointed; for, upon trial it appeared that what I had inferred only from analogical reafoning refpecting the force of aqueous vapour above the boiling point, actually happened with that from ether above the faid point. And ether is a much better fubject for experiment in this cafe than water, becaufe it does not require fo high a temperature.

with the fyphon barometer.

I took a barometer tube of 45 inches in length, and having fealed it hermetically at one end, bent it into a fyphon fhape, making the legs parallel, the one that was clofe being nine inches long, and the other 36. Then conveyed two or three drops of ether to the end of the clofed leg, and filled the reft of the tube with mercury, except about 10 inches at the open end. This done, I immerfed the whole of the fhort leg containing the ether into a tall glafs containing hot water; the ether thus expofed to a heat above the temperature at which it boils, produced a vapour more powerful than the atmofphere, fo as to overcome its preffure and raife a column of mercury befides, of greater or lefs length according to the temperature of the water. When the water was at 147° the vapour raifed a column of 35 inches of mercury, when the atmofpheric preffure was 29.75: fo that vapour from ether of 147° is equivalent to a preffure of 64.75 inches of mercury; agreeing with the force of aqueous vapour of 257°, according to the preceding eftimation: in both cafes the temperatures are 45° above the refpective points of ebullition. In all the temperatures betwixt 102° and 147° the forces of ethereal vapour correfponded with thofe of aqueous vapour, as per table, betwixt 212° and 257°. I could not reafonably doubt of the equality continuing in higher temperatures; but the force increafes fo faft with the increafe of heat, that one cannot extend the experiments much farther without tubes of very inconvenient lengths. Being defirous however to determine the force

Experiments in which the reaction of included air was uled as the meafure of force.

force of the ethereal vapour experimentally up as high as 212°, I contrived to effect it as follows:—Took a syphon tube such as described above, only not quite so long, and filled it in the manner above mentioned, with ether and mercury, leaving about ten inches at the top of the tube vacant; then having graduated that part into equal portions of capacity, and dried it from ether, I drew out the end of the tube to a capillary bore, cooled it again so as to suffer the internal atmospheric air to be of the proper density, and suddenly sealed the tube hermetically, thus inclosing air of a known force in the graduated portion of the tube. Then, putting that part of the tube containing ether into boiling water, vapour was formed which forced the mercurial column upwards and condensed the confined air, till at length an equilibrium took place. In this way I found 8.25 parts of atmospheric air of the force 29.5 were condensed into 2.00, at the same time a perpendicular column of 16 inches of mercury in addition pressed upon the vapour. Now the force of elastic fluids being inversely as the space, we have 2.00 : 29.5 : : 8.25 : 121.67 inches = the force of the air within; to which adding 16 inches, we obtain 137.67 = the whole force sustained by the vapour, measured in inches of mercury. The force of aqueous vapour, at the same distance beyond the boiling point, or 322°, is equal to 137.28, per table. Thus it appears that in every part of the scale on which experiments have been made, the same law of force is observable with the vapour of ether as of water.

Experiments on Spirit of Wine.

Force of vapor of spirit of wine.

By boiling a small portion of the spirit I used (about one cubic inch) in a phial, the thermometer stood at 179° at the commencement; but by continuing the ebullition it acquired a greater heat. The reason is, the most evaporable part of the spirit flies off during the process of heating, and the rest being a weaker compound, requires a stronger heat. The true point of ebullition, I believe, was nearly 175°.—The force of the vapour from this spirit at the temperature of 212°, I found both by an open syphon tube and one hermetically sealed with atmospheric air upon the mercurial column, as with ether, to be equal to 58½ inches of mercury.

This

This rather exceeds the force of aqueous vapour at an equal diſtance from the boiling point; but it is no more than may be attributed to unavoidable little errors in ſuch experiments. In a barometer tube the ſpirituous vapour at 60°, over the mercury, depreſſes the column about 1.4 or 1.5 inches; which is ſomething leſs than the due proportion; one cauſe of this may be the evaporability of ſpirits, which in operating on ſmall quantities, quickly diſſipates part of their ſtrength.

Experiments on Liquid Ammonia.

Force of vapor of liquid ammonia,

Liquid ammonia or volatile alkali, the ſpecific gravity of which was .9474, boiled near 140°; in the barometer a ſmall quantity depreſſed the mercury 4.3 inches in the temperature of 60°. In higher temperatures it did not produce a proportional depreſſion; becauſe the moſt volatile part of the compound, expanding in the vacuum of the barometer, leaves the reſt more watery, and conſequently its vapour muſt be weaker; eſpecially when the portion uſed is confined to a drop or two.

Muriate of Lime.

of muriate of lime.

Put a portion of liquid muriate of lime over the column of mercury in a barometer. The boiling point of the muriate was found by experiment to be 230°. At 55° the depreſſion was ,22 of an inch:

at 65°—.30
— 70°—.40
— 95°—.90

all which nearly agree with the forces of aqueous vapour 13° below the reſpective temperatures.

Mercury and Sulphuric Acid.

Forces of the vapour of mercury and ſulphuric acid.

Mercury boils by my thermometer at 660°, and ſulphuric acid of the ſpecific gravity 1.83, boils at 590°. It is very difficult to determine the preciſe force of vapour from theſe liquids in any temperature under 212°; becauſe at ſuch great diſtance from the boiling point the vapour is ſo weak as to be in effect almoſt imperceptible, Following the general law, the vapours of theſe fluids ought to be of the force .1, mercury at 460°. and ſulphuric acid at 390°.—Col. Roi makes the expanſion of 30 inches mercury by 180° of heat = .5969 or .5651; and in a barometer the expanſion in the ſame

ſame circumſtances is .5117; the differences are .0852 and .0534 which ſhould meaſure the effective force of mercurial vapour of 212°, nearly. This is in all probability too much; as it is next to impoſſible to free any liquid entirely from air; and if *any* air enter the vacuum, it unites its force to that of the mercurial vapour.

That the force of vapour from ſulphuric acid, in low temperatures, is exceedingly ſmall, will appear from the enſuing ſection.

SECTION II.

On Vapour in Air.

Effect of the expanſion of vapour in air. The experiments made by a ſmall tube, ſtopped by a moveable plug of mercury.

The experiments under this head were made with manometers, or ſtraight tubes of different lengths, hermetically ſealed at one end, of $\frac{1}{15}$ inch internal diameter, and their capacities divided into equal portions. A drop or two of the liquid, the ſubject of experiment, was conveyed to the bottom or ſealed end of the tube; the internal ſurface was then dried by a wire and thread, and atmoſpheric, (or any other air) was admitted into the tube, upon which a column of mercury was ſuſpended of $\frac{1}{10}$ of an inch, or of 30 inches, leſs or more, according to the nature of the experiment. By immerſing the end of the manometer, containing the air thus circumſtanced, into a tall glaſs veſſel containing water of any temperature, the effect of the vapour in expanding the air could be perceived. It was firſt indeed neceſſary to determine the increaſe air unaffected by any liquid (except mercury) would obtain by increaſe of temperature: that was done, as will be particularly ſhewn in the next eſſay.* The expanſion of all elaſtic fluids, it ſeems probable, is alike or nearly ſo, in like circumſtances; 1000 parts of any elaſtic fluid expands nearly in a uniform manner into 1370 or 1380 parts by 180° of heat.

General law of expanſion of air and vapour together. The ſpace at any temperature is directly as the preſſure and inverſely as the preſſure leſs the force of the vapour.

It will be unneceſſary to repeat in detail the numerous experiments made on the various liquids in all temperatures from 32° to 212°; as the reſults of all agree in one general rule or principle, which is this: let 1 repreſent the ſpace occupied by any kind of air of a given temperature and free from moiſture; $p =$ the given preſſure upon it, in inches of mercury; $f =$ the

* Philoſophical Journal, Vol. III. page 130.

force

force of vapour from any liquid in that temperature, in vacuo; then, the liquid being admitted to the air, an expanſion enſues, and the ſpace occupied by the air becomes immediately, or in a ſhort time $= 1 + \frac{f}{p-f}$; or, which is the ſame thing, $= \frac{p}{p-f}$.

Thus in water for inſtance:

Let $p=30$ inches,

$f=15$ inches, to the given temp. 180°.

Then, $\frac{p}{p-f} = \frac{30}{30-15} = 2$, for the ſpace; or the air becomes of twice the bulk.

If the temperature be 203°, $f=25$, and the ſpace becomes ſix times as large as at firſt.

If $p=60$ inches

$f=30$ inches to the given temperature 212°; then the ſpace $= \frac{60}{60-30} = 2$; or water under the preſſure of 60 inches of mercury, and at the temperature of 212°, produces vapour which juſt doubles the volume of air.

If ether be the inſtance: let the temperature be equal 70°; then $f=15$; and ſuppoſe $p=30$; in this caſe the colume of air is doubled; that is, ether of 70° being admitted to any portion of air, doubles its bulk.

The expanſion of hydrogenous gas and atmoſpheric air by the vapour of water is the ſame for every temperature.

Sulphuric acid does not expand atmoſpheric air to any ſenſible amount by the heat of boiling water.

The theory of theſe facts is evident upon the principles laid down in the former eſſay: for inſtance; let it be required to explain the experiment with water of 212° under a preſſure of 60 inches. Here the air was condenſed into the ſpace 1 by the preſſure of 60 inches; but being expoſed to water of 212°, a vapour aroſe from it equal in force to 30 inches; the air therefore expanded till its force alſo became = to 30 inches, which was effected by doubling its volume: then the vapour preſſing with 30 inches force and the air alſo with 30 inches force, the two together ſupport the preſſure of 60 inches and the equilibrium continues. In ſhort, in all caſes the vapour ariſes to a certain

certain force, according to temperature, and the air adjufts the equilibrium, by expanding or contracting as may be required.

The notion of a chemical affinity fubfifting between the gafes and vapours of different kinds, cannot at all be reconciled to thefe phenomena. To fuppofe that all the different gafes have the fame affinity for water might indeed be admitted if we could not explain the phenomena without it; but to go further, and fuppofe that water combines with every gas to the fame amount as its vapour in vacuo; or in other words, that the elafticity of the compound fhould be exactly the fame as if the two were feparate, is certainly going far to ferve an hypothefis. These facts do not agree with the notion of chemical affinity between gas and vapour.

Befides, we muft on this ground fuppofe that all the gafes have the fame force of affinity for any given vapour; a fuppofition that cannot be admitted as having any analogy to other eftablifhed laws of chemical affinity.

(To be continued.)

XIV.

Defcription of the Portable Furnace conftructed by Dr. Black, and fince improved. In a Letter from Mr. Accum.

To Mr. NICHOLSON.

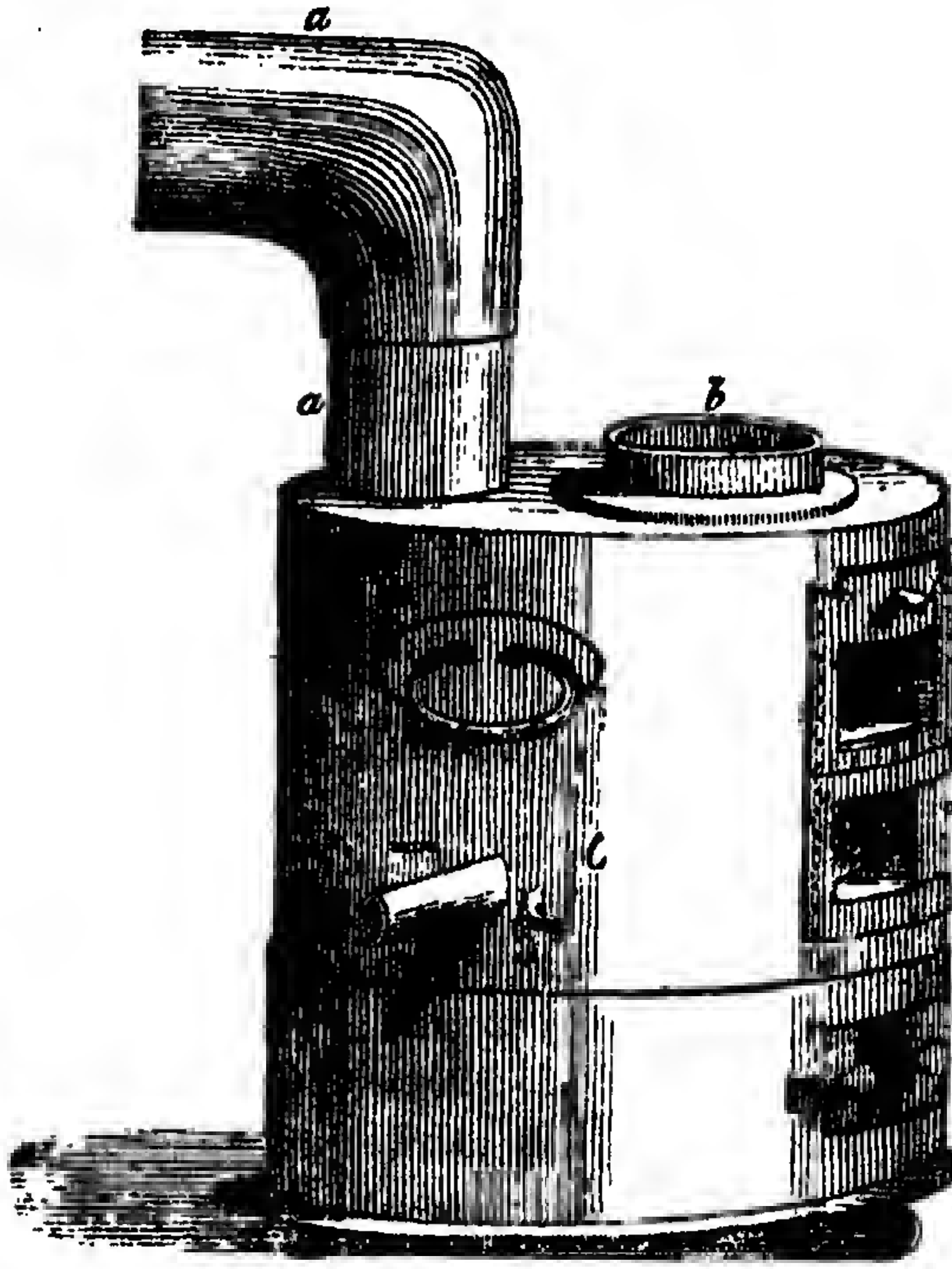
DEAR SIR,

IN my Syftem of Practical Chemiftry, Vol. II. p. 357, I have given a Defcription and Drawing of a Portable Univerfal Furnace, which in the practice of my profeffion I found the beft furnace for all chemical operations whatever which require like aid. The number of furnaces which I have caufed to be made for different philofophers of that kind, and the ufeful hints which I have received from different quarters, have materially improved it, that I flatter myfelf, whether a defcription of this furnace would not be acceptable to your readers, particularly to thofe who have no accefs to the laboratory of the operative chemift; for thofe who are familiar with practical chemiftry will readily allow, that a furnace capable of producing a very low and very intenfe heat is one of the moft requifite and moft indifpenfible inftruments of all the apparatus of chemiftry. The Defcription of the portable furnace in chemical operations.

 great

Defcription of the portable furnace in chemical operations.

great advantage of this furnace (*which was firft invented by Dr. Black, and improved by others*) above all others I am acquainted with, confifts in confuming as little fuel as poffible, in producing quickly, if required, a very intenfe heat—in regulating expeditioufly, and at pleafure, its intenfity—in applying it as directly, and as fully as poffible, to the fubftances upon which it is intended to act—and moreover in enabling the operator to perform his operations in the clofet, or in any other place, without the rifk of endangering the conflagration of the furrounding objects, which were not meant to be expofed to the action of heat.



This portable univerfal furnace is made of ftrong wrought iron plates. It is lined with bricks, bedded in fire-proof loam. Its height without the chimney *a a* is two feet. The inner diameter of the cylindrical fire-place meafures ten inches. The body of the furnace is elliptical; in its upper part a circular hole is cut, for receiving an iron fand-pot *b* which may occafionally be removed and exchanged for an iron plate. In the front of the furnace there are three openings over each other, furnished with fliding doors, and fitted

with

with stoppers made of crucible ware. The lower opening *c*, is the ash-pit of the furnace; it is composed of two register plates, sliding backwards and forwards in grooves, in order to diminish, or enlarge the opening for regulating the heat, by admitting or excluding air at pleasure. In the side of the furnace a hole is cut, furnished with a stopper and door, for passing a tube through the fire-place of the furnace; an expedient very necessary for a variety of chemical processes, such as exhibiting the decomposition of water, alcohol, oils, &c. for the preparation of phosphuret of lime, for passing gases over ignited bodies, &c. In either of the openings in front of the furnace, a muffle may be placed for performing the process of cupellation of gold, silver, &c. or, the neck of a retort (placed on a stand in the body of the furnace) may be passed through it, for distillation by the naked fire; for procuring gases which require a high degree of heat, &c. If the iron sand-pot *b* be removed, and a circular plate properly lined with fire-clay be placed in its room, the furnace becomes converted into a wind-furnace; the fuel is then to be introduced through either of the openings in front. The iron plate at the top has a hole in the centre, furnished with a stopper, to enable the operator to inspect his process at pleasure. If the iron-pot be placed inverted on the opening of the furnace, it forms a dome, and it then becomes a reverberating furnace. The iron-pot when filled with sand, or water, placed in its proper situation, serves as a sand or water-bath, for the processes of distillation by means of glass retorts, for evaporations, sublimations, digestions, &c. Coake and charcoal are the best fuel, this mixture burns without smoke, and gives a strong uniform and permanent heat; charcoal and common coal, or coal only, does likewise very well. The elbow of the chimney *a* may be directed into that of the fire-place of any apartment.

Description of the portable furnace in chemical operation.

The furnace is furnished with castors, and may therefore be easily moved, according to the convenience of the operator. I am, SIR,

Your most obedient servant,

FREDERICK ACCUM.

Old Compton Street, Soho,
Nov. 21, 1803.

XV.

Obſervations on the Structure of the Tongue; illuſtrated by Caſes in which a Portion of that Organ has been removed by Ligature. By EVERARD HOME, *Eſq. F. R. S.**

Introduction.

PHYSIOLOGICAL inquiries have ever been conſidered as deſerving the attention of this learned Society; and, whenever medical practitioners, in the treatment of diſeaſes, have met with any circumſtance, which threw light upon the natural ſtructure or actions of any of the organs of the human body, or thoſe of other animals, their communications have met with a favourable reception.

Importance of a ſafe means of removing part of the tongue.

The following obſervations derive their real importance from offering a ſafe and effectual means of removing a portion of the tongue, when that organ has taken on a diſeaſed action, the cure of which is not within the reach of medicine; and, as the tongue, like many other glandular ſtructures, is liable to be affected by cancer, it becomes of no ſmall importance that the fact ſhould be generally known. In a phyſiological view, they tend to ſhow, that the internal ſtructure of the tongue is not of that delicate and ſenſible nature which, from its being the organ of taſte, we ſhould be led to imagine.

Its ſtructure

The tongue is made up of faſciculi of muſcular fibres, with an intermediate ſubſtance met with in no other part of the body, and a vaſt number of ſmall glands; it has large nerves paſſing through it; and the tip poſſeſſes great ſenſibility, fitting it for the purpoſe of taſte,

Whether the ſenſe of taſte is confined entirely to the point of the tongue, and the other parts are made up of muſcles fitted for giving it motion; or whether the whole tongue is to be conſidered as the organ, and the ſoft matter which pervades its ſubſtance, and fills the interſtices between the faſciculi of muſcular fibres, is to be conſidered as connected with ſenſation, has not, I believe, been aſcertained.

ſuppoſed to be very delicate.

The tongue, throughout its ſubſtance, has always been conſidered by phyſiologiſts as a very delicate organ; and it was believed, that any injury committed upon it would not

* Philoſ. Tranſact. 1803, p. 205.

4 only

only produce great local irritation, but alſo affect, in a violent degree, the general ſyſtem of the body. This was my own opinion, till I met with the following caſe, the circumſtances of which induced me to ſee this organ in a different point of view.

A gentleman by an accident which it is unneceſſary to deſcribe, had his tongue bitten with great violence. The immediate effect of the injury was great local pain; but it was not attended with much ſwelling of the tongue itſelf, nor any other ſymptom, except that the point of the tongue entirely loſt its ſenſibility, which deprived it of the power of taſte: whatever ſubſtance the patient eat was equally inſipid. This alarmed him very much, and induced him to ſtate to me the circumſtances of his caſe, and requeſt my opinion. I examined the tongue a fortnight after the accident. It had the natural appearance, but the tip was completely inſenſible, and was like a piece of board in his mouth, rendering the act of eating a very unpleaſant operation. I ſaw him three months afterwards, and it was ſtill in nearly the ſame ſtate.

Accident of the tongue very much bitten, and rendered inſenſible,

From this caſe it appears, that the tongue itſelf is not particularly irritable; but the nerves paſſing through its ſubſtance to ſupply the tip, which forms the organ of taſte, are very readily deprived of their natural action; this probably ariſes from their being ſofter in texture than nerves in general, and in that reſpect, reſembling thoſe belonging to the other organs of ſenſe.

There was another circumſtance in this caſe which very particularly ſtruck my attention, *viz.* that a bruize upon the nerves of the tongue, ſufficient to deprive them of the power of communicating ſenſation, was productive of no inflammation or irritation in the nervous trunk, ſo as to induce ſpaſms, which too commonly occur from injuries to the nerves belonging to voluntary muſcles. I am therefore led to believe, that the nerves ſupplying an organ of ſenſe, are not ſo liable to ſuch effects as thoſe which belong to the other parts of the body.

without inflammation or irritation to produce ſpaſms.

The ſmall degree of miſchief which was produced, and the readineſs with which the nerves had their communication completely cut off, were to me new facts, and encouraged me, in

Caſe of fungous excreſcence of the tongue.

in the following cafe of fungous excrefcence from the tongue, which bled fo profufely as at times to endanger the patient's life, and never allowed him to arrive at a ftate of tolerable health, to attempt removing the part by ligature.

which when removed was followed by violent hæmorrhage.

John Weymouth, eight years of age, was admitted into St. George's hofpital, on the 24th of December, 1800, on account of a fungous excrefcence on the right fide of the anterior part of the tongue, which extended nearly from the outer edge to the middle line at the tip. It appeared, from the account of his relations, that the origin of this fungus exifted at his birth, and had been increafing ever fince. He had been a year and a half under the care of the late Mr. Cruikfhank, who had removed the excrefcence by ligature round its bafe; but, when the ligature dropped off, a violent hæmorrage took place, and the excrefcence gradually returned, Attempts were made to deftroy it by cauftic; but hæmorrhage always followed the feparation of the floughs; fo that, after ten trials, this mode was found ineffectual. It was alfo removed by the knife, ten different times, but always returned.

From this hiftory I was led to believe, that the only mode of removing the difefae was taking out the portion of the tongue upon which it grew. This was a cafe in which I felt myfelf warranted in making an attempt out of the common line of practice, to give the patient a chance of recovery; and, from the preceding cafe, having found that preffure on one part of the tongue produced no bad confequences on the other parts, I was led to remove the excrefcence in the following manner.

The portion of tongue removed by ligature.

On the 28th of December, I made the boy hold out his tongue, and paffed a crooked needle, armed with a double ligature, directly through its fubftance, immediately beyond the excrefcence. The needle was brought out below, leaving the ligatures; one of thefe was tied very tight before the excrefcence, the other equally fo beyond it, fo that a fegment of the tongue was confined between thefe two ligatures, in which the circulation was completely ftopped. The tongue was thin in its fubftance; and the boy complained of little pain during the operation. Thirty drops of laudanum were given to him immediately after it, and he was put to bed. He fell afleep, continued to dofe the greater part of the day, and was fo eafy the next day as to require no particular attention.

On

On the fifth day from the operation, the portion of tongue came away with the ligatures, leaving a floughy furface, which was thrown off on the 11th day, and was fucceeded by a fimilar flough; this feparated on the 15th day. The excavation after this gradually filled up; and, on the 20th day, it was completely cicatrized, leaving only a fmall fiffure on that fide of the tongue.

(To be concluded in our next.)

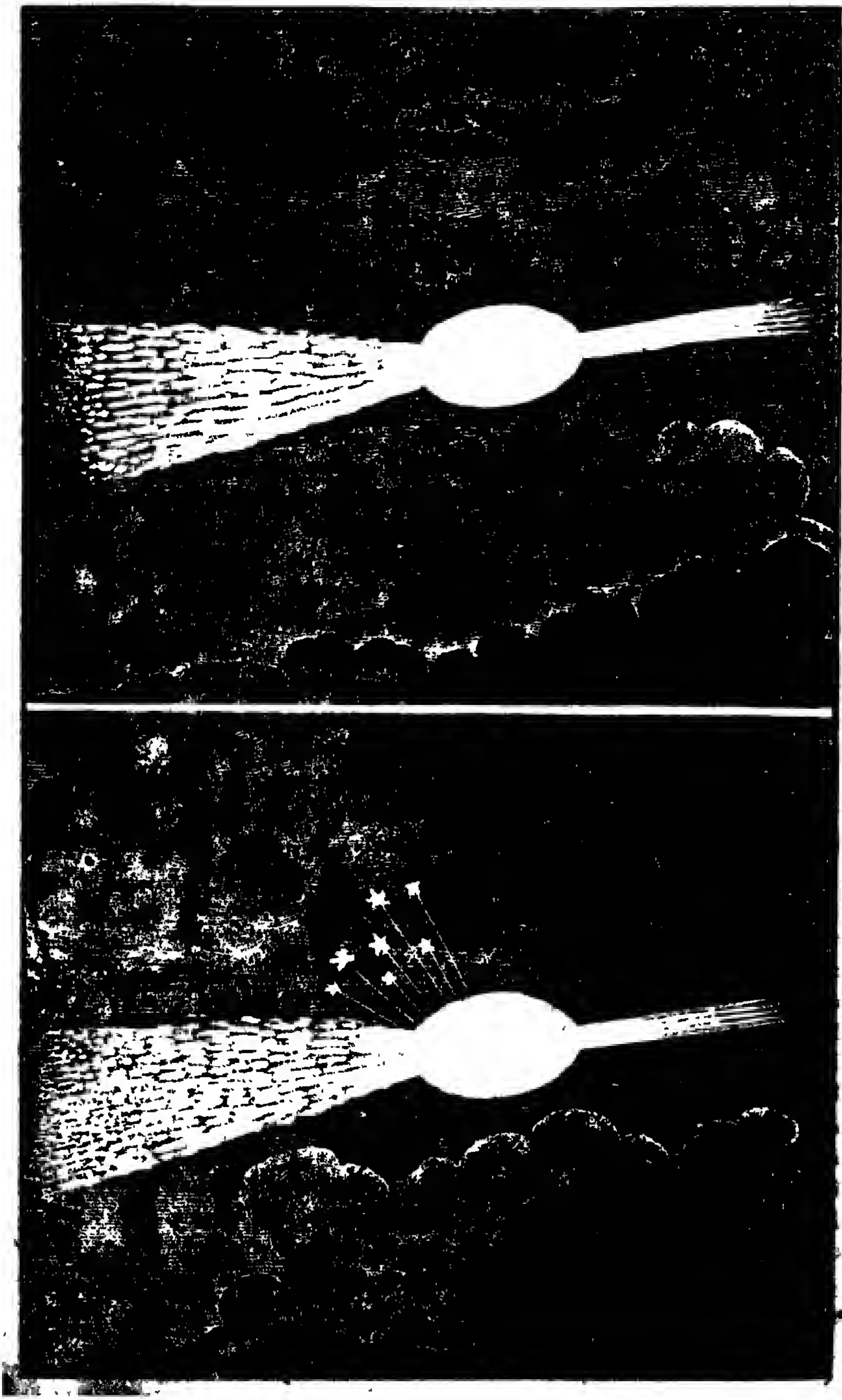
XVI.

Some Account of the large fiery Meteor which appeared on the fixth of laft Month (November.)

ON Sunday evening at half paft eight, on the fixth of laft month, I was fuddenly furprized with an illumination refembling that of a flafh of lightning, but more permanent. The windows of the room in which I was fitting face the fouth, and were not clofed either by the fhutters or curtains, but only by venetian blinds; through which fome of the company afferted that they faw a large globe of fire moving to the weftward. My back was towards the window, fo that I faw only the light which appeared confiderably blue, and feemed to laft two, or perhaps three feconds. The bluenefs in all probability was not more than that of day-light, which, when contrafted with the light of candles, has a lively blue tinge.

Account of a fiery meteor.

A fcientific friend of mine, who has favoured me with a fketch from which the annexed drawings were taken, was walking up Princes Street Soho, and turned upon the fudden appearance of light, when he faw the meteor paffing rapidly over St. Ann's church yard, having the appearance of an oblong or elliptical folid, with a fhort radiating eruption from its preceding part, and numerous fparks diverging from its hinder part. He compares it to the burning of a combuftible matter in oxigen, and faw it burft into many fparks, which inftantly went out and left extreme darknefs. Its direction feemed to be to the fouthward of a line fuppofed to crofs Princes Street at right angles, which eftimate would give a courfe about W. S. W. This gentleman faw the great meteor of Auguft 18, 1783, which was then round, and he thinks the prefent quite as large as that.



Another friend informs me, that he saw it from a station in St. James's Park, near the Queen's House, rising above the horizon, in the east towards Westminster-abbey, and that it passed over St. James's Park, and part of the Green Park, where it was lost behind a cloud. He thinks it remained in sight for a much longer time than two or three seconds, and that it did not move in a straight line. The course by his observation would be about W. N. W. Account of a fiery meteor.

Another person who saw it burst, speaks of the portions falling down like the sparks of a rocket.

That it passed as most large fiery meteors seem to do, in the superior part of our atmosphere is probable from the general facts. A gentleman on Hampstead Heath, beheld the country suddenly illuminated, and clearly saw Harrow Steeple, which is eight miles distant in a strait line, and it was also seen from Dartford in Kent, until it became obscured by a cloud; but I had no further particulars respecting it.

Much difficulty must arise in estimating either the course, direction, or elevation of meteors of this kind, which appear when totally unexpected, and are gone before the mind can enter into any course of reasoning or estimate. We are fully occupied with the impressive sensation they produce, and have scarcely any other means of obtaining a conjecture respecting positions and altitude, but by repeating our observations on the spot. On this subject the reader may consult an excellent paper by Sir Charles Blagden in the LXXIV. volume of the Philosophical Transactions.

The lower sketch shews the manner of explosion into smaller pieces.

XVII.

A First Memoir on coloured Shadows *. By Cit. J. H. Hassenfratz.

Shadows are generally but erroneouſly counted black.

WE are accuſtomed to conſider as black, the ſhadow formed by an opaque body, which intercepts the light falling on a white paſteboard, although in reality, nothing is more difficult to obtain than black ſhadows, becauſe it appears, that to procure a black ſhadow, the light muſt abſolutely be a point, and no ſort of reflected light muſt reach the illuminated ſurface.

Phenomena of ſhadows.

On a ſurface illuminated by a light which has magnitude, the ſhadow is always accompanied by a coloured penumbra. Whenever the illuminating body is at a greater diſtance than a metre from the body enlightened, and that which intercepts the light, is nearly five decimetres from this body, the penumbra and ſhadow have diſtinct colours, which are in a great meaſure dependent upon the nature of the combuſtible body which yields the light.

The blue ſhadow at ſun-riſe and ſun-ſet, is not different from the general laws of ſhadows.

Since the period when Leonardo da Vinci noticed the blue ſhadow which is perceived in the morning at ſun-riſe, and in the evening at ſun-ſet, this ſhadow has been attended to, and naturaliſts have endeavoured to explain its production, as forming a phenomenon peculiar and diſtinct from thoſe of other ſhadows. Neverthleſs, this ſhadow has no particularity; all the ſhadows which are examined are coloured, even thoſe produced by the noon-tide ſun of a clear ſummer's day, which are commonly called *black ſhadows*. The coloured ſhadows are produced by two or more diſtinct lights or by the ſeparated parts of the ſame light which act differently.

Variety of coloured ſhadows.

Nothing is more diverſified than the colours of ſhadows; on noticing them with attention, we remark among them all the priſmatic colours: we diſtinguiſh red, orange, yellow, green, blue, violet ſhadows, more or leſs mixed with black.

The ſubject is to be purſued beyond this Memoir.

The object of this firſt Memoir, is to make known the great variety of coloured ſhadows which may be diſtinguiſhed

* From Journal de L'Ecole Polytechnique, Tom. IV. p. 272.

and

and which we have obſerved, and alſo the circumſtances which give riſe to them. Other memoirs which we purpoſe to preſent after this, if the inſtitute ſhall think the ſubject worthy of its attention, will contain the ſeries of obſervations we have made to aſcertain the cauſes which produce the coloration of ſhadows in a great number of caſes.

Diviſion of the ſubject.

We ſhall divide this Memoir into three parts: the firſt will have for its object the colour of the ſhadows produced by the reunion of the light of the atmoſphere, and that of the ſun, or of the light of the atmoſphere and an artificial light, but in thoſe circumſtances wherein only a ſingle ſhadow is perceptible: the ſecond will contain a deſcription of the coloured ſhadows produced by the light of the atmoſphere combined with reflected lights, and ſometimes with the direct light of the ſun: finally, the third will elucidate the coloured ſhadows formed on a body illuminated by two lights, natural and artificial.

Part the Firſt.

Obſervations of the nature of coloured ſhadows,

If, near a white ſurface enlightened by the ſun and the light of the atmoſphere, a black body is placed which intercepts the ſolar rays, there will be ſeen on the plane a ſhadow which varies from a greeniſh blue to a violet black, connecting through the blue and the violet. The colour of the ſhadow depends on the ſtate of the atmoſphere, the latitude of the place, the meridional and northern declination of the ſun, and the time elapſed between its riſing and its paſſage to the meridian, and from the latter to its ſetting.

as produced by change of ſeaſon,

When the ſky is clear, the colour of the ſhadow at ſun-riſe, at Paris, varies between blue with a ſlight tinge of green to violet blue. The firſt days of Nivoſe, the ſhadow is greeniſh-blue; the firſt days of Germinal, blue; the firſt days of Meſſidor, indigo with a violet tinge; the firſt days of Vendemiaire, the ſhadow becomes again blue to return to a greeniſh blue the firſt of Nivoſe.

as produced by the meridional height of the ſun,

If on a clear day, when the ſun is on the equator, the variation of the colour of the ſhadow, if noticed from the inſtant of ſun-riſe to its paſſage over the meridian, it will be obſerved that this colour is blue at ſun-riſe; that at each elevation of the luminary above the horizon, the blue changes; that it becomes indigo, violet; that in the end it blackens, and that when the ſun is on the meridian, the ſhadow is of a blackiſh violet.

Every

as produced by the meridian sun at different periods of the year,

Every day from the first of Nivose to the first of Messidor, the solar shadow offers different tints, at the moment when this luminary passes the meridian. The first days of Nivose, the shadow is violet, a little blackish; it increases in blackness daily to the first Messidor; at this period, the shadow is violet-black.

as produced by difference of latitude.

If the coloured shadow of the rising sun is observed on a clear day, at the same period, and in different latitudes, it will be seen to vary from violet-blue to green, going from the equator to the pole.

The coloured shadow observed at the beginning of Nivose, at sun-rise, from Messina to Skalhot in Iceland, is, at Messina, light indigo; at Vienna and at Paris, blue, with a slight green tinge; at London, Berlin, Copenhagen, Edinburgh, a more distinct green tinge; at Petersburg, a little more green; finally at Drontheim and at Skalholt, a greenish tint.

At the same period, at noon, the shadow varies, between Messina and Skalholt, from black slightly tinged with violet to violet.

Result of the observations.

The comparison of the colour of the shadow of the sun, with its situation in the different places where it is observed, naturally leads to this first conclusion, that it is different in the ratio of the intensity of its light, compared with that of the atmosphere, in fact, the rising sun, on the first of Nivose, having a feeble light in comparison with that of the atmosphere, the shadow is a greenish blue; as it rises above the horizon, the intensity of its light increases, and the shadow becomes blue, indigo, violet: finally, when the sun is on the meridian, its light has acquired its greatest intensity, and the shadow blackens, preserving nevertheless a violet tinge.

Comparing in the same manner the colour of the shadow observed each day of the year at Paris, as well as that observed in each latitude on the same day, it will be seen, that it changes from green to violet-black, according to the intensity of the light acquired by the sun; and when in winter the sun, being but little elevated above the horizon, appears red, from the feeble light which penetrates the light mists existing in the air, the solar shadow is green, sometimes a fine grass green.

Experiments with an artificial light which is varied,

To satisfy ourselves whether the colour of the shadow depended on the relation of the light compared to that of the atmosphere, we placed the light of a lamp near a white surface, illuminated solely by the light of the atmosphere; when this lamp

lamp was five decimetres from the enlightened plane, the ſhadow cauſed by an opaque black body was blue. On bringing the lamp nearer the colour of the ſhadow changed ſucceſſively; from blue it changed to indigo, from indigo to violet, and the violet blackened gradually. When the lamp was very cloſe, the ſhadow was of a violet black colour, exactly ſimilar to that produced by the light of the ſun on a clear ſummer's day.

This experiment ſucceeds very well on days when the ſun is hid by clouds, all bodies are then illuminated by the light of the ſky.

A ſimilar reſult may be obtained by the inverſe method, that is to ſay, by illuminating the ſurface with an artificial light of conſtant intenſity, and ſucceſſively increaſing the intenſity of the light of the ſky. With an artificial light which is fixed.

If in a dull morning, before the appearance of twilight, a white ſurface is illuminated by the light of a lamp placed at five decimetres diſtant from the enlightened ſurface, the ſhadow of an opaque black body placed at a ſmall diſtance from the ſurface, is black very ſlightly violaceous. As ſoon as the twilight appears, the tint changes, the intenſity of the violet increaſes. As the day brightens, the violet of the ſhadow effaces the black tint; at length the ſhadow becomes violet, indigo, and is blue when the light of the day is completely developed.

Repeated experiments with a taper or a candle, have given the ſame reſults.

It follows, from the obſervations detailed, that every ſhadow, formed on a body illuminated, at the ſame time, by the light of the atmoſphere and the direct light of the ſun; or by the light of the ſky and an artificial light, ſuch as a lamp, a taper, a candle, is coloured in all caſes wherein the light of the ſun, or the artificial light is intercepted by an opaque black body; and that the colour of the ſhadow varies from green to violet black, in the ratio of the intenſity of the light of the ſun, or the artificial light, compared with that of the atmoſphere. But on what depends the colour of the atmoſphere? what cauſes that variation of colour in the ratio of the comparative intenſity? This is what we ſhall examine in another Memoir. General reſult.

(To be concluded in our next.)

SCIENTIFIC

SCIENTIFIC NEWS.

Observations on St. John's Wort. By Cit. BAUNACH.*

The juice of St. John's wort affords a red colour,

ST. JOHN's wort, *hypericum perforatum*, is a resinous plant, the tops and flowers of which contain a juice soluble in water, alcohol, and vinegar. With the former two it gives a blood-red colour, with the last a fine bright crimson. When combined with [mineral?] acids or metallic solutions, it affords a yellow colour, which proves, that it contains two colouring matters, one, the red, more soluble than the other.

and also a yellow.

Red most soluble.

Mode of dyeing with it.

To dye linen, woollen, silk, or cotton yellow, it is sufficient to put them into a bath, the water of which is duly impregnated with the juice of this plant, with a certain quantity of a mordant. The best mordant for this colour is alum combined with a suitable portion of potash. The stuff must be left in the bath some time; for the durability of the colour, and the shade produced, depend chiefly on the time of continuance in the bath, the quantity of the mordant, and the degree of heat employed. When but little mordant is used, the tint is a bright yellow; by increasing it the colour is made to incline to green; and on adding a solution of tin in nitro-muriatic acid, it assumes rose, cherry, and crimson hues, all with a fine lustre. Alum alone does not answer well, the addition of potash being essential. This decomposes the alum, precipitates its earth, dissolves a considerable portion of it, and this alcaline salt with an earthy base becomes the true mordant in the process; the more because the colouring principle resides in a substance almost purely resinous.

Various tints produced by it.

Alum and potash form the proper mordant.

Stains paper yellow,

The juice of St. John's wort, united with the mordant here mentioned gives a fine yellow colour to paper; and as it produces the same effect on leather, it may be employed with advantage for dying sheep and other white skins.

and dyes leather.

Contains tannin.

The plant contains a considerable quantity of tannin, as I have been convinced by means of the solution of glue, and other experiments made for this purpose.

Sulphate of iron converts it into a concrete resin.

On dropping a little solution of sulphate of iron into the juice of St. John's wort, a blackish brown precipitate is formed, which has the property of absorbing oxygen, becoming at length insoluble in water, and assuming the characters of a concrete resin.

* From the *Annales de Chimie.*

Having

Affords an agreeable water, but no essential oil.

Having distilled a certain quantity of the plant with water, the product had a powerful and agreeable smell, but I could not discover the least trace of essential oil on it.

The juice does not dissolve in oils, but unites with resins.

Mode of combining them.

The juice of St. John's wort does not dissolve either in expressed or in essential oils, but it unites very well with resins. For this purpose, the juice of the plant must be dried; which may be done very conveniently by expressing it into earthen plates, and placing these in an oven some time after the bread is drawn; it must then be powdered, and will readily combine with turpentine by rubbing them together in a brass mortar warmed. This resin, thus saturated with the juice, may be mixed with oils, either essential or expressed; and on combining it with oil of olives, the oil of St. John's wort of the shops may be formed, which, thus prepared, possesses evident virtues. If it be incorporated with linseed oil, and a small portion of oil of turpentine be added, a fine red varnish is produced, which may be advantageously employed for coating articles of furniture made of wood.

Oil of St. John's wort.

Makes a fine red varnish for wood.

MR. RICHARD KNIGHT, who is well known to the philosophical world for the very complete Magazine of Chemical Apparatus of all kinds he has for several years past established in Foster-Lane, has favoured me with a letter, in which he very satisfactorily shews that the instrument in *Plate* X. of our last number, was not invented by Mr. Accum, but by Mr. W. H. Pepys, about three years ago, and has ever since been an article on sale in the catalogue of his warehouse. The title to Mr. Accum's paper was written by myself, as almost all the titles are; and I was led to call him the inventor from the tenor of his paper. Immediately after the publication of last number, and before I had received any letter from Mr. Knight, Mr. Accum observed to me, that he is not the inventor, and that he first saw the instrument described in a German paper. At the same time therefore that I have the pleasure to give Mr. Pepys the undisputed right to a contrivance which, in point of utility and convenience, is of considerable value, I do not see any moral error that needs correction.

ACCOUNT

ACCOUNT OF NEW BOOKS.

The Edinburgh New Dispensatory; containing, I. *The Elements of Pharmaceutical Chemistry:* II. *The Materia Medica; or, the Natural, Pharmaceutical and Medical History of the different Substances employed in Medicine:* III. *The Pharmaceutical Preparations and Compositions; including complete and accurate Translations of the 8vo. Edition of the London Pharmacopœia, published in* 1791; *Dublin Pharmacopœia, published in* 1794; *and of the New Edition of the Edinburgh Pharmacopœia, published in* 1803. *Illustrated and explained in the Language, and according to the Principles of Modern Chemistry. With many new and useful Tables, and several Copper-plates, explaining the new System of Chemical Characters, and representing the most useful Pharmaceutical Apparatus. By* ANDREW DUNCAN, *Jun. M. D. Fellow of the Royal College of Physicians, and Royal Society of Edinburgh, and Associate of the Linnæan Society of London. 8vo,* 720 *Pages, and* 6 *Plates. Edinburgh,* 1803.

Duncan's Edinburgh New Dispensatory.

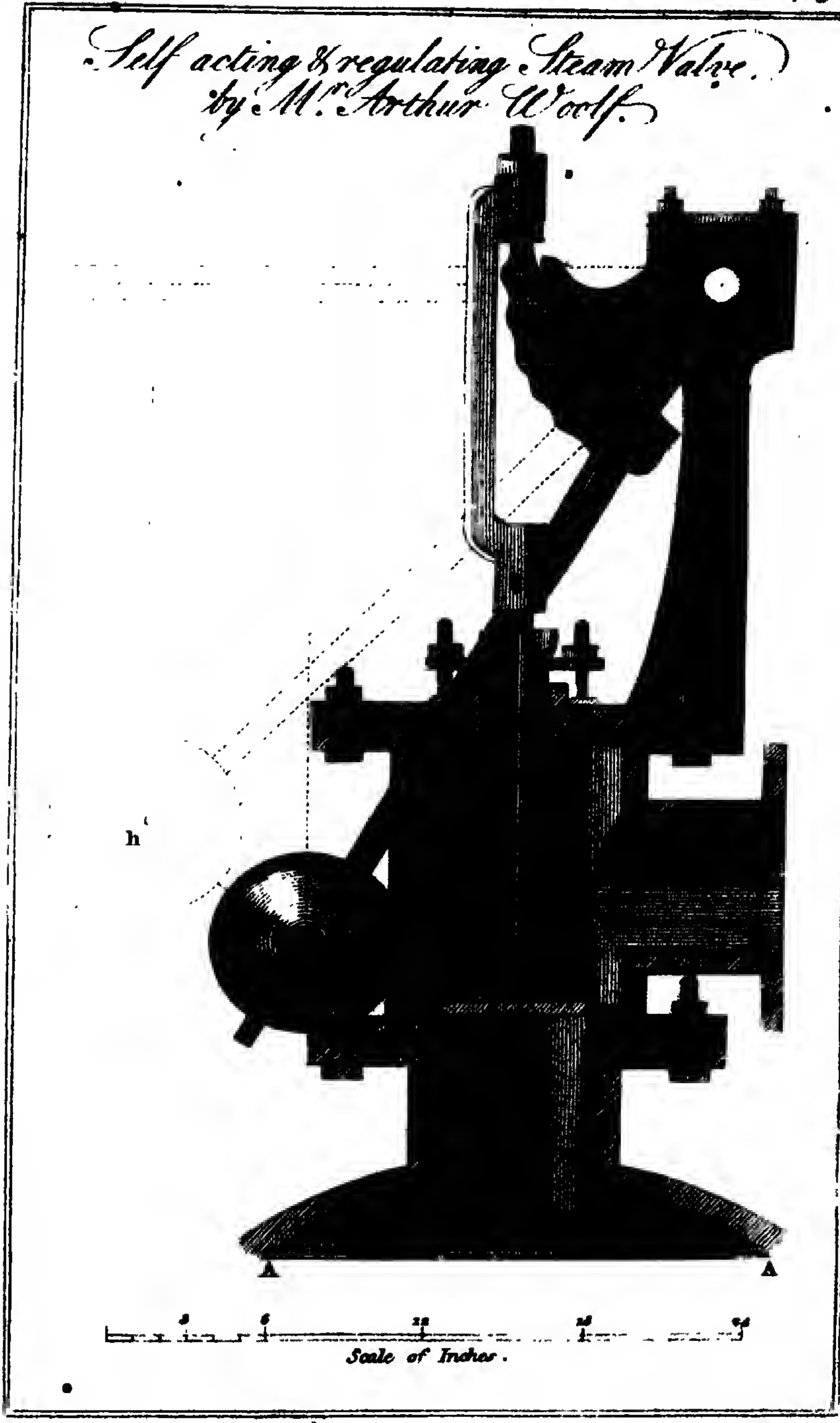
THE copious title page of this work informs the reader what he has to expect in this new edition of a stock book, the excellent foundation of which was laid by Dr. Lewis in 1753. The translation of the Dublin Pharmacopœia must be an acceptable addition, and the introductory Epitome of Modern Chemistry, a knowledge of which is indispensable to those who would understand Pharmacy as a science, or practise it with advantage as an art; was loudly called for by the many improvements made of late years in this branch of science.

It would take up too much room to give the titles of the several new tables, which are all useful: and Dr D. appears to have availed himself of every thing in the field of modern discovery, or in the best foreign Pharmacopœias, that was consistent with the plan of the work.

Accum's lectures on chemistry.

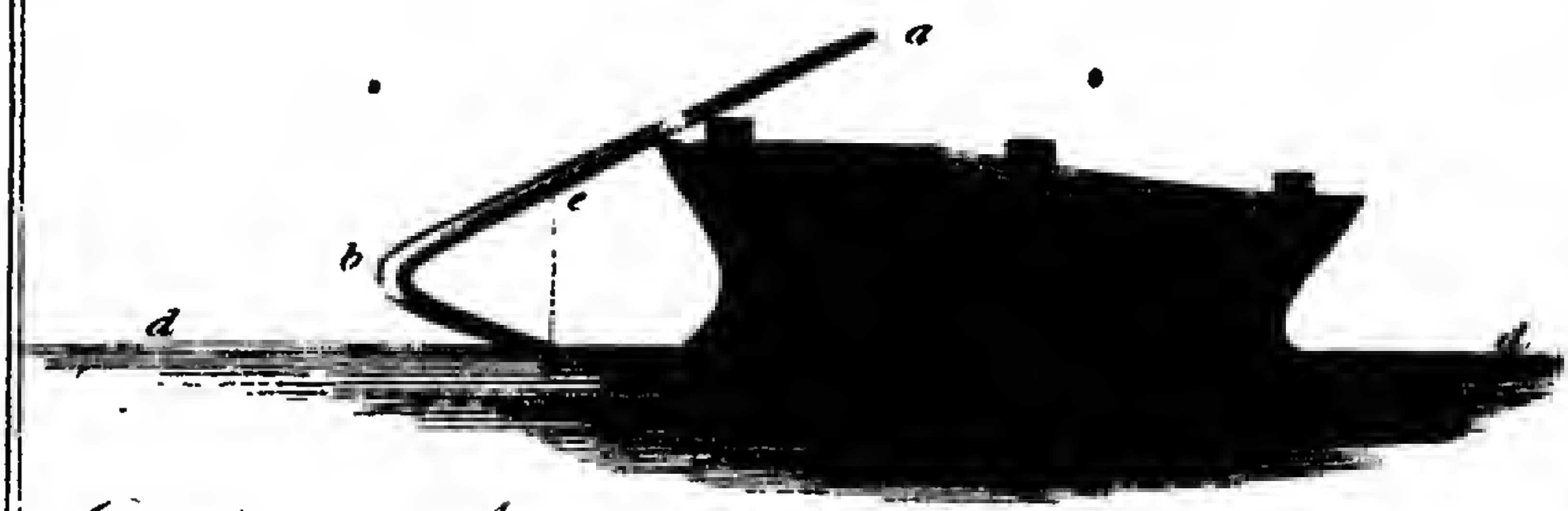
MR. ACCUM, who has resigned his gratuitous service as Assistant Chemical Operator in the Royal Institution, is about to enter on a Course of Lectures on Practical Chemistry, and its Application to Agriculture, Arts, and Manufactures. They will comprehend distinct Series of Lectures on Popular Chemistry, Operative Chemistry, Mineralogical Chemistry, Agricultural Chemistry, and Galvanism.

Self acting & regulating Steam Valve.
by Mr. Arthur Woolf.



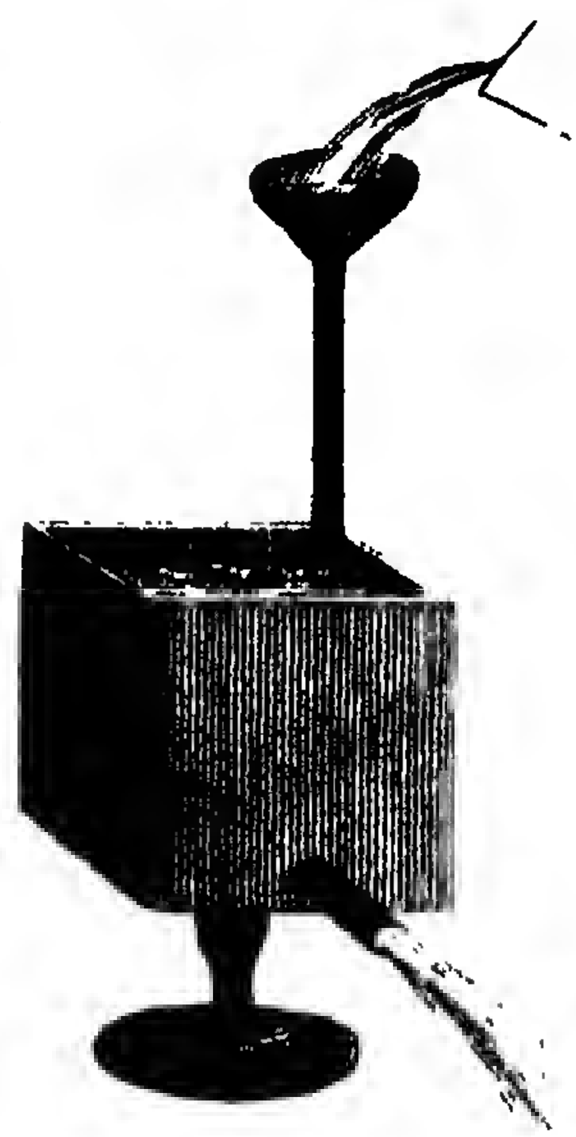
Engraved by Mutlow

Dr. Wollaston on Terrestrial Refraction.

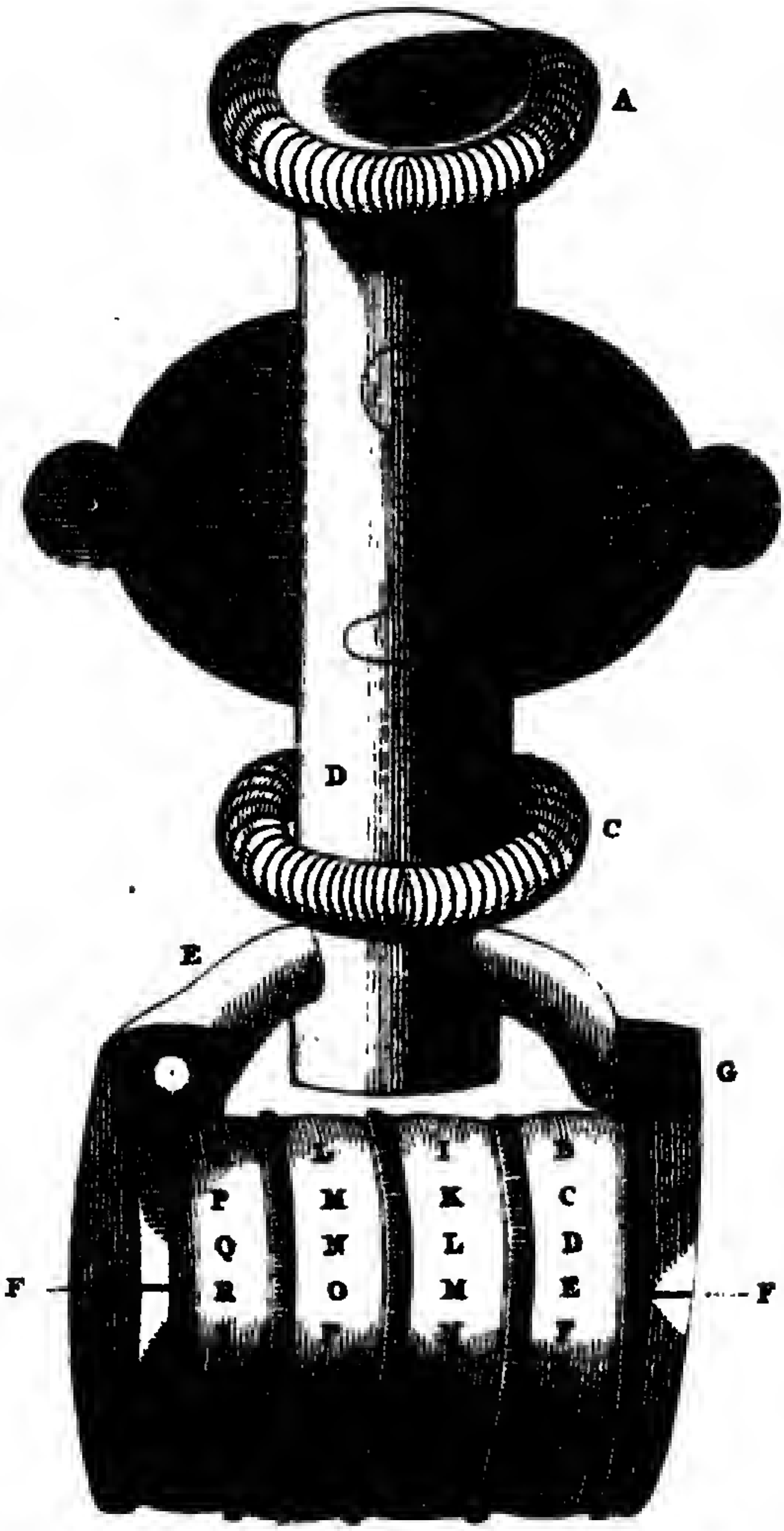


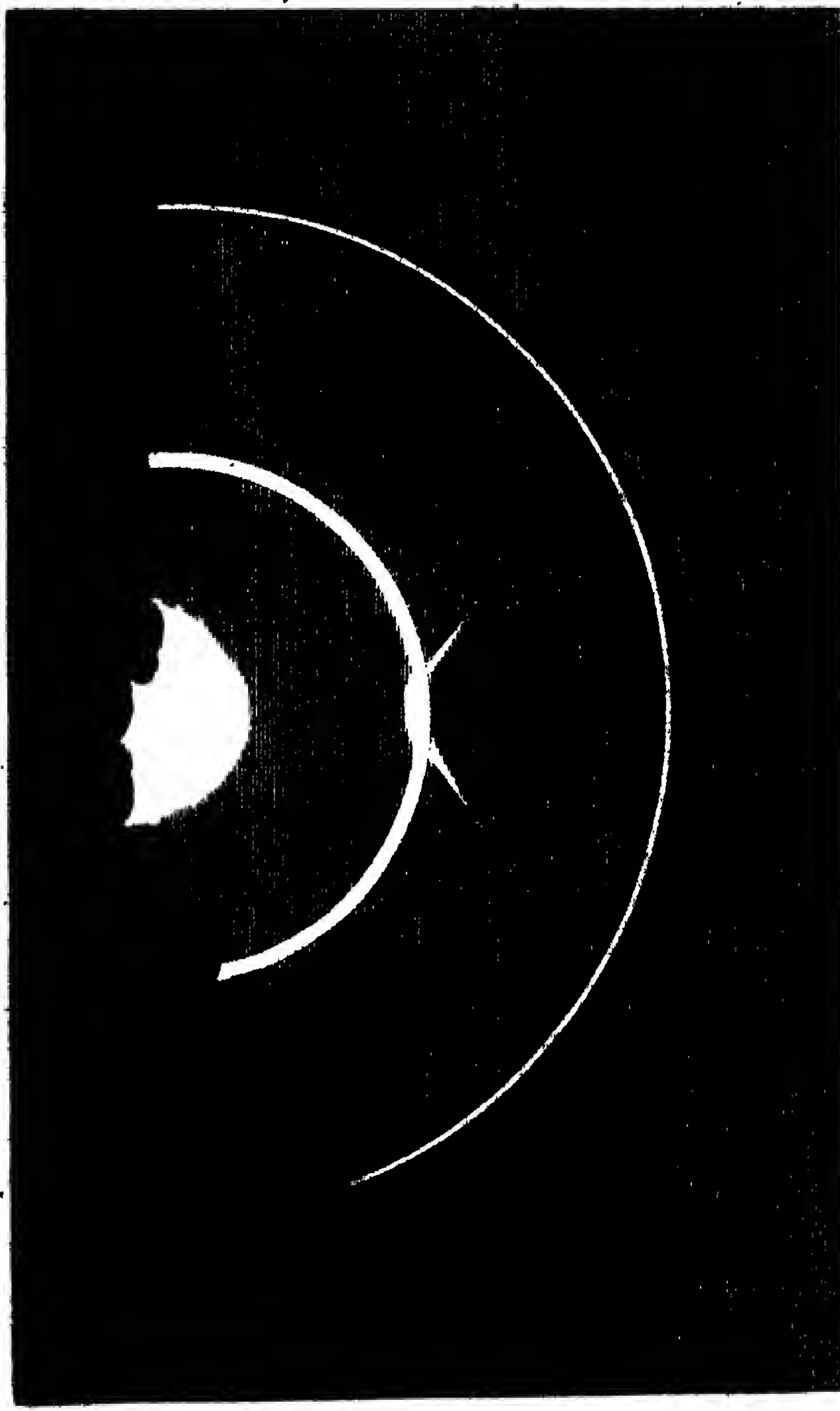
Dr. Young's Apparatus to Illustrate the Doctrine of Preponderance

Experiment of the Velocity of Water thro' a Vertical Pipe.



Lock of Combination by M. Regnier.





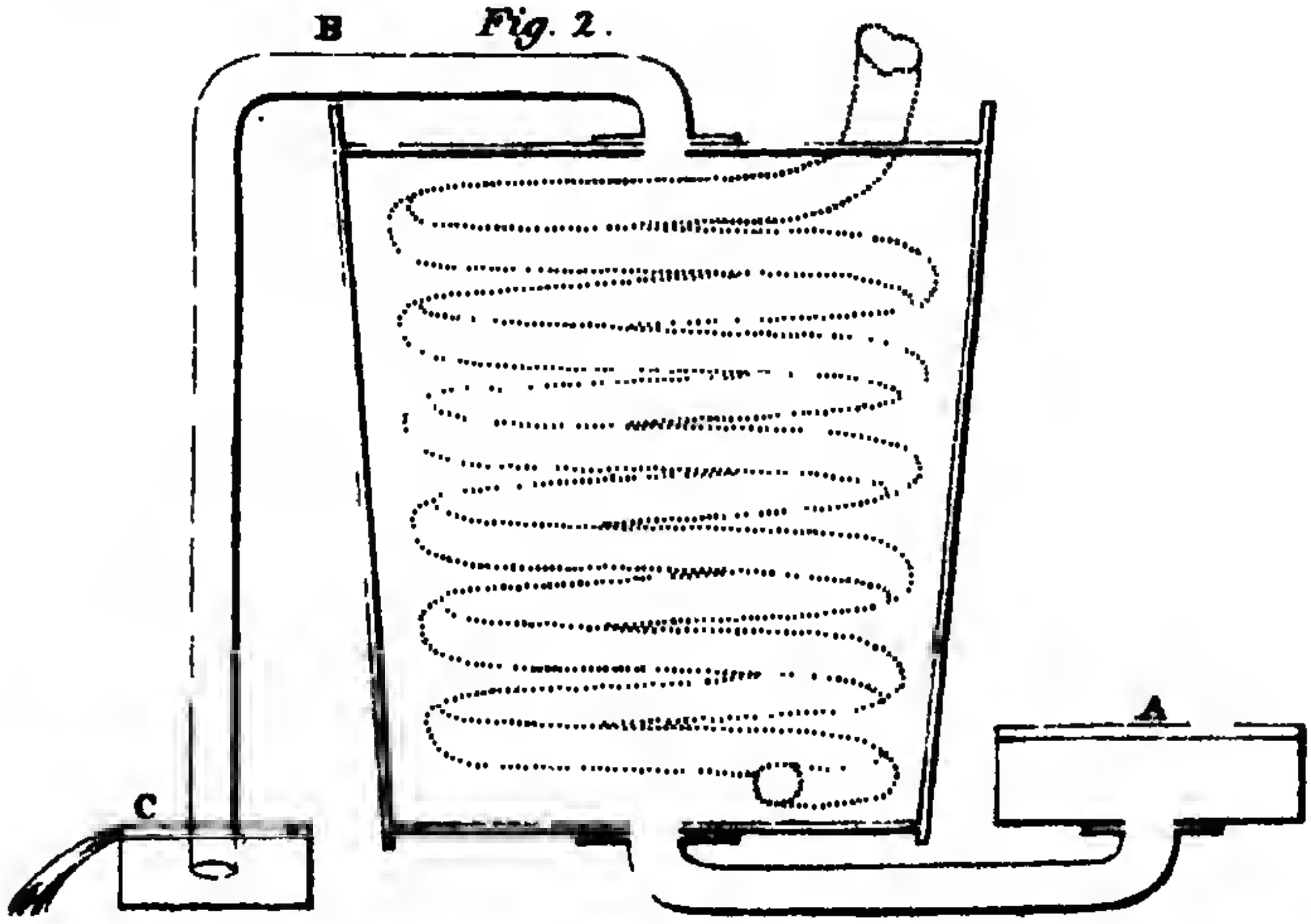
Mahon & Russell C.

Dr. Herschel on the magnified Image of the same Star, at different times.

Fig. 1.

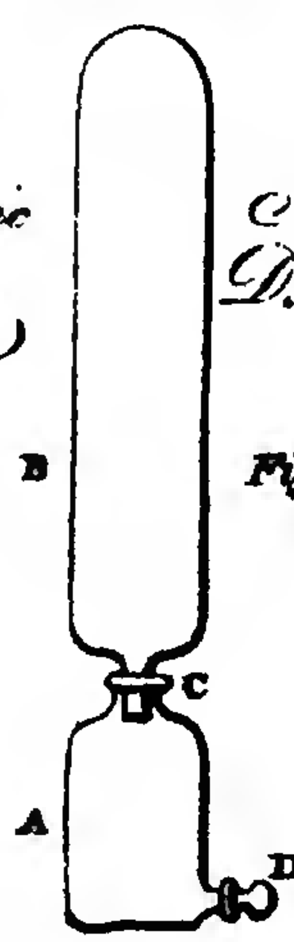
Sir A. N. Edelcrantz's method of easily raising Water in worm-tubs, condensers, &c.

Fig. 2.



Eudiometric Apparatus: By Dr. Hope.

Fig. 3.



INDEX.

A.

INDEX.

G.

H.

I.

K.

L.

La

S.

V.

W.

Y.

Z.

END OF THE SIXTH VOLUME.

Printed by W. Stratford, Crown-Court, Temple-Bar.